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Psychological Review

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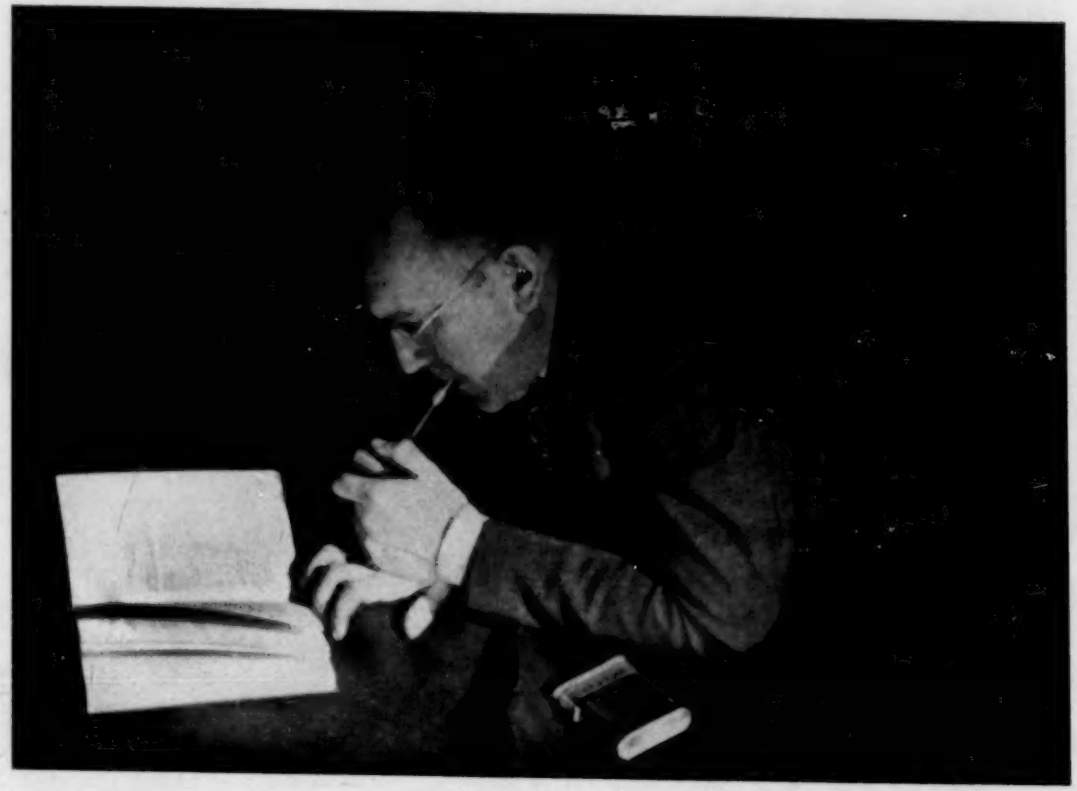

Willard L. Valentine

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THE PSYCHOLOGICAL REVIEW

WILLARD LEE VALENTINE

1904–1947

One day a young man, Willard Lee Valentine, came into my office at Ohio Wesleyan University to discuss the possibility of transferring from engineering to psychology. Although he was then a senior and had completed only the first course in general psychology, his training in mathematics and physics, plus his intelligence and capacity for work, soon made him the outstanding student in the department. He became the model for other students of that period.

The sudden death of Dr. Valentine on April 5 brought to an abrupt end this promising career. His passing was a shock to his many friends who had been associated with him not only in psychology but in the whole realm of science. Although he had only recently taken over the task of editor of *Science*, he had already begun to make himself felt in the advancement of that important journal. Preceding his appointment to *Science*, he had been for a number of years treasurer of the American Psychological Association and business manager of the journals published by that organization. In this capacity he was able to demonstrate his organizing ability. The duties of the treasurer were multiplying and the complexities of the financial problems connected with an ever-expanding group of journals were increasing. Dr. Valentine showed remarkable skill in meeting these problems. It was through his efficiency and projection of future possibilities that he put the operation of the journals on such a sound financial basis that the American Psychological Association was able to offer its members and associates club rates.

Like that of many other men who have forged ahead in this world, Dr. Valentine's origin was a humble one. He was born in Chillicothe, Ohio, on December 2, 1904. None of his family was college trained, but it was the kind of family one likes to think of as a solid American family. From time to time he mentioned particularly his mother as influencing and directing his thinking in his early years.

In 1921 he entered Ohio Wesleyan University on a scholarship. There he was granted his A.B. degree in 1925 and his M.A. degree in 1926. He continued on as an assistant until 1928. Meanwhile, under a coöperative program, he carried on graduate work toward the doctor's degree at Ohio Wesleyan and Ohio State Universities. During these years he demonstrated the keen interest and enthusiasm in a variety of projects in psychology which were to mark his later work. It was not unusual, when I arrived at the laboratory in the morning, to find him still there after he had worked all night. He always worked intensively; in fact, he crowded into his forty-two years as much as some men get done in sixty years.

His first published research in 1927 dealt with binaural beats and the shift of the phantom sound. This grew out of G. W. Stewart's earlier studies in which he had demonstrated mathematically that the phantom should travel in a circle. Using one of Stewart's original subjects, Valentine clearly showed that the phantom was so unstable that it travelled from ear to ear through any course that might be suggested. Following these experiments, he turned to visual perception in the white rat. Although he considered this a Gestalt problem, the publication of his article, 'Visual perception in the white rat,' in 1928 did not result in a vindication of his hypothesis.

In 1928 he went to Ohio State University as an instructor and in 1929 he completed his doctor's dissertation, 'A study of learning curves,' under the direction of A. P. Weiss. He was then appointed assistant professor, and in the fall of 1929 I went to Ohio State University as supervisor of the first course in psychology. We were thus brought together again for another decade.

The creation of a separate area which dealt exclusively with the teaching of the first course in psychology was the brain child of Dean George F. Arps. He had charged Valentine and me with the task of developing it. Valentine's duties were chiefly the organization of the laboratory for beginning students. He had very little equipment and in those early days a great deal of his time and energy were spent in begging the administration for money with which to buy apparatus, writing progress reports, improvising, and building apparatus out of whatever he could get his hands on. But as occurred so many times when he took over a new job, things soon began to happen. Apparatus started to arrive, demonstrations were mapped out and, under his guidance, students were beginning to learn about the scientific method.

Perhaps his most unique contribution at this time was the planning of motion pictures as supplementary material for the course. To show the beginning student how psychologists work, he made the film, 'A Trip through the Psychology Laboratory.' It contained principally sample shots of various projects of graduate students at that period. We were interested in the maturation of structure and function. Valentine made several motion pictures showing the week-by-week development of locomotion and manipulation in the infant, based largely on the Yale studies. The film that attracted most attention, however, was one he produced in collaboration with Dr. Frank Stanton. It was designed to illustrate the various factors in visual space discrimination. It was not unusual, when this film was first exhibited, to hear non-laboratory trained teachers of psychology remark that this was the first time they clearly understood space perception as they themselves taught it. Before he had finished with this phase of his contribution to the young department, he had made or secured films which illustrated practically all the subject matter of the beginning course.

It was not Valentine's purpose to make artistic films, but to depict as accurately as possible what is seen in the laboratory. He wanted the student to see what he is intended to see and not to be distracted by incidental characteristics that had nothing to do with the subject at hand. For this reason, he found most sound movies made by professional motion picture companies unsatisfactory because of the incidental sound effects. He also emphasized the importance of thorough discussion by the class of the film shown. The film was not for entertainment but was a more efficient substitute for the tradi-

tional laboratory demonstration. Not only did we find these films valuable teaching aids in our beginning psychology courses at Ohio State, but other universities, too, became interested in them with the result that there was a wide sale of them.

During this time he was still developing his laboratory course for beginners. He had worked out all sorts of exercises, experiments, and demonstrations which were within the scope of the inexperienced beginner. Most of the directions, background, history, and so forth, he presented in mimeographed form, or orally. To make the course more efficient and to have in one place all the conglomeration of material needed for his laboratory work, he compiled it in a manual which he published under the title of 'Laboratory Manual.'

Feeling the need of a supplementary reading text which would make available to the beginning student a more thorough acquaintance with important research in psychology, Valentine published in 1931 his *Readings in Experimental Psychology*. In this text he compiled, with each investigator's permission, a revised account of important original, psychological investigations. In his attempt to simplify the presentation for the beginner, he demonstrated his genius for picking out the very core of the article and presenting it in a simple, concise manner. Many psychologists, including many of the authors of the original articles, commented that these condensations were more readable than the originals. Yet, Valentine was nonplussed by the fact that some universities used the text for graduate students and so few adopted it for beginning students.

His second book he felt was more successful. In the earlier years he had assumed that the beginning student was capable of analyzing what he reads, of deriving principles, and of applying these principles to life situations. He soon found, however, that the function of the teacher is to train the student in these functions, not merely to give him the data of psychology. This was brought out in an analysis of the first course published in the *Journal of Applied Psychology* in 1935. Profiting from this experience in 1938, he approached the subject matter in his second book, *Experimental Foundations of General Psychology*, in a different manner from what he had used in his other writing. This text was designed to give the student some insight into the procedures of experimental work concerning behavior and by simple analysis point out what generalizations seem reasonable in any specific report. It proved to be a valuable supplementary text.

Despite the fact that the duties connected with the first course were so demanding of his time and energy, Valentine seemed ever to have more time and energy for activities in other areas. The more that was going on around him, the better he liked it. The picture of him hunched over his desk, calmly writing, in the midst of a complete hubbub is a very vivid one. He did research, he directed research, he taught advanced courses. He became well-known in other departments. He was sought out for advice and counsel by members of departments which were not directly connected with his own work.

Out of all this experience he developed into a mature, intellectually keen, self-confident man. He was an excellent teacher and a great inspiration to his students. He had an exceptional ability to make clear to the student the abstruse problems of psychology. His personality seemed to permeate the department. It was therefore with very

great regret that we at Ohio State University parted with him in 1940. He became head of the department at Northwestern University where he carried over the same ability to organize which he displayed at Ohio State.

Scientist, teacher, executive—Dr. Valentine was all of these and something more. He was a stimulating force in science. We will miss the impact of his intellect and industry upon the problems and development of psychology.

F. C. DOCKERAY
Ohio State University

A PROPOSED QUANTIFICATION OF HABIT STRENGTH [1]

BY CLARK L. HULL, JOHN M. FELSINGER, ARTHUR I. GLADSTONE AND HARRY G. YAMAGUCHI

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INTRODUCTION

The central factor of a science of behavior naturally is behavior itself. It inevitably follows that the central problem of a systematic natural-science approach to behavior is that of determining from given antecedent events and conditions what behavior will follow. Such a determination amounts to a prediction, *i.e.*, a statement of the behavioral potentiality ($_sE_R$) which lies in the relevant antecedent events and states. Now, the critical immediately preceding event in the case of reaction evocation appears to be stimulation (S), and the critical immediately preceding states involved are (1) the strength of the habit ($_sH_R$) and (2) the primary motivation or drive (D), *e.g.*, hunger.[2] There is some reason to believe that the potentiality of the evocation of a reaction in a simple, *i.e.*, a non-competitional, situation will turn out one day to be multiplicative in nature. More specifically, it is believed that when these three functions are suitably quantified it will be found that

$$_sE_R = {}_sH_R \times D. \qquad (1)$$

On this assumption, if the stimulation and the drive at the several trials are kept constant as they ordinarily are during experimental learning processes, the responses obtained at the successive trials will be substantially a function of habit strength alone, quite as is ordinarily assumed. Actually, of course, the habit strength will throughout such a series be multiplied by a constant value of drive which, strictly speaking, makes it a case of reaction potential.[3] The purpose of the present paper is primarily to propose a methodology for quantifying a sequence of habit strengths (reaction potentials with motivation constant) which results from a simple learning situation of this kind.

One major difficulty in the way of satisfactorily quantifying reaction potential is that its objective manifestations take so many different forms. These manifestations, while measurable, are usually measured in different units and rarely or never, even when measured in the same units, yield results which are comparable. Moreover, in some cases, *e.g.*, muscular

[1] This is the second study in the series by the present authors. The experimental work, as described in the previous study (7), was performed by Mr. Felsinger, the statistical analysis was carried out by Mr. Yamaguchi and Mr. Gladstone, and Mr. Hull supervised the investigation and prepared the manuscript in frequent consultation with the other three authors. The senior author takes entire responsibility for the methodology and the interpretations here given. The authors are indebted to Professors Irvin L. Child, Mark A. May, and Kenneth W. Spence for a critical reading of a preliminary draft of the manuscript. Dr. H. O. Gulliksen also contributed a valuable suggestion.

[2] Secondary motivation is here neglected, partly in the interest of expository simplification but mainly because of uncertainty as to its exact status.

[3] On the other hand, if the $_sH_R$ of equation 1 is kept constant and the D is varied, the resulting series of reaction potentials will be substantially a function of primary motivation alone, *i.e.*, the product of a variable D multiplied by a constant value of $_sH_R$.

contraction, several different aspects of the response, such as latency, amplitude, and force, can be employed as indicators of what at bottom is presumably the same thing. A number of phenomena recognized as associated quantitatively with reaction potential are listed in Table 1. All these presumably possess a monotonic relationship to reaction potential, and all are evidently increasing functions of it except reaction latency and number of errors which obviously have a decreasing or inverse functional relationship. But ${}_{s}E_{R}$ itself clearly is not necessarily a certain number of degrees of arc, a certain number of grams, millimeters, cubic centimeters, seconds, or errors.

Moreover, all quantifications of reaction potential, however empirically manifested, should be expressible on the same scale and in terms of the

TABLE 1

A tabular summary of various objectively observable and quantifiable manifestations of reaction potential (${}_{s}E_{R}$), the aspect of the manifestation quantified, the unit ordinarily employed in this quantification, and typical references to experimental work.

Phenomenon	Aspect of phenomenon quantified	Customary unit	Typical references
1. Conditioned secretion	Saliva	Drops or cubic centimeters	24; 1
2. Galvanic skin reaction	Electric potential	Degrees of arc moved by galvanometer	2; 18
3. Striated muscular contraction	Intensity of contraction	Grams	8
4. Striated muscular contraction	Amplitude of resulting movement	Centimeters	22; 17
5. Striated muscular contraction	Reaction latency	Seconds	12; 9; 28; 30
6. Change in size of optic pupil	Length of cross-section	Millimeters	19
7. Breathing amplitude	Chest expansion as recorded by pneumograph	Millimeters	10; 21
8. Pulse rhythm	Variability in amplitude of pulse beats as graphically recorded	Oscillometer units	29
9. Occurrence of erroneous response	Ratio of errors to total relevant trials	Probability of erroneous reaction ($\bar{p}$)	14; 31
10. The mere occurrence of the response, whatever it is	Ratio of response occurrence to total suitable stimulations	Probability of a given response occurrence (p)	6; 37; 27
11. Experimental extinction	Unreinforced reaction evocations	Number of extinctive responses (n)	36; 25
12. Vocal squeak	Latency and intensity	Seconds and amplitude of graphic record	5; 16

same unit. This principle at once renders questionable for quantification the cubic centimeter because this is peculiar to salivary secretion and does not directly apply, for example, to the galvanic skin reaction. It renders questionable for this purpose intensity of muscular contraction because this will not directly apply to salivary secretion. It rules out in the same sense the number of degrees of arc moved by a galvanometer, because most manifestations of ${}_{S}E_{R}$ are not primarily electrical in nature. It rules out the intensity and the amplitude of muscular contraction because many reactional manifestations are not of a muscular nature and so cannot be measured in grams or centimeters. It rules out number of erroneous reactions because some processes, such as the salivary secretion and the galvanic skin reaction, are not associated with competing reactions capable of displacing them as is ordinarily the case with reactions called errors.

A second major difficulty in the way of the proposed quantification is that habit strength, drive, and reaction potential are never available as such to direct observation of the scientist (**20**, p. 21). In a word they are logical constructs much as are the atoms and molecules of physics and chemistry. It is instructive to note in this connection that in spite of the fact that individual atoms are never observed, much less placed in a balance and weighed, their masses have been determined with great accuracy by indirect means. It is true, of course, that the fact that atomic phenomena have been quantified successfully by indirect means does not imply that the quantification of habit phenomena will be achieved, much less that any particular methodology of such quantification will be successful. The facts of atomic quantification do negate, on the other hand, the major premise prevalent in certain psychological circles (**38**) that indirect methods of the quantification of theoretical constructs is absurd because *a priori* impossible. True, the process of atom quantification has consumed much time, together with great labor and profound thought. On the other hand, the quantification of reaction potential as such has hardly yet been proposed, much less seriously attempted. Until the problem is attacked in a vigorous and protracted manner it cannot be said whether it will be more or less difficult than have been parallel tasks in the physical sciences. Meanwhile we submit that the quantification of atoms, even of uranium or plutonium, is of no greater ultimate significance for the welfare of man than is that of behavior potentiality.

THE PRINCIPLES UPON WHICH THE PROPOSED QUANTIFICATION IS BASED

In order to facilitate the reader's comprehension of the statement of methodology which follows, it has been thought desirable to state the major principles upon which the methodology is based and which serve as a kind of background for it, together with a general outline of their operation in the present situation.

1. It is assumed with confidence that with distributed trials and minimum work load, habit strength (${}_{S}H_{R}$) increases up to a maximal asymptote as the reinforcements (N) increase.[4]

2. It has been shown experimentally (**7**) that reaction latency (${}_{S}t_{R}$) decreases to a minimal asymptote as the reinforcements (N) increase.

3. From (1) and (2) it follows that habit strength (or reaction potential, D constant) has an inverse mono-

[4] This is believed to hold where reaction latency is not specifically trained to be slow, as in delayed conditioned reactions (**20**, p. 337).

tonic functional relationship to reaction latency.

4. It follows from (3), other things equal, that if reaction latency B is found to be less than reaction latency A, then the momentary reaction potential (${}_{S}\dot{E}_{R}$) corresponding to B must be greater than that corresponding to A.

5. It is a universal empirical observation that reaction latency where the related reaction potential (${}_{S}E_{R}$) is presumably constant, *e.g.*, at the ${}_{S}t_{R}$ asymptote, varies from instant to instant, and so from trial to trial.

6. It follows from (3), (4), and (5) that the momentary reaction potential (${}_{S}\dot{E}_{R}$) must vary from instant to instant (**20**, p. 319).

7. Further, it is specifically assumed that the spontaneous variability in the momentary reaction potential (${}_{S}\dot{E}_{R}$) approximates the normal form of distribution (**20**, p. 304 ff.), ${}_{S}E_{R}$ remaining constant.[5]

8. It is a statistical commonplace that the differences between two sets of uncorrelated values which are themselves distributed normally will be normally distributed (**13**, pp. 218, 219).

9. It is self-evident that if a considerable number of reaction latencies, all secured after a given organism has reached its latency asymptote, are available and these are drawn at random and compared, it will be found that except for occasional cases of equality about half of the latencies chosen first will be shorter than those chosen second, and about half will be longer.

10. It follows from (6), (7), and (8) that the two halves of the distribution of reaction-latency differences of (9) correspond to the two symmetrical wings of a normal or Gaussian distribution of reaction-potential differences.

[5] Certain indirect evidence bearing on the soundness of this assumption will be presented later in the present paper (p. 250 ff.).

11. It is also a statistical commonplace (**13**, pp. 218, 219) that if a random comparison of two normal uncorrelated distributions A and B occurs and significantly more than 50 per cent of the B's are found to be greater than the A's, the mean of the B's must be greater than the mean of the A's.

12. By (10), (11), and related statistical principles, it follows that if the latencies of two groups of ${}_{S}t_{R}$'s, A and B, are compared and more than 50 per cent of the B's are shorter than the A's, this not only implies that the ${}_{S}E_{R}$ of the B's must be greater than is that of the A's, but that this difference has a determinate magnitude. The logic behind the quantification of the difference in reaction potential is illustrated in Fig. 1. For example, suppose that the A's are reaction latencies from a habit with two reinforcements and the B's are from a habit of three reinforcements, and that 40 of the comparisons out of a total of 59 show the B's to be shorter, *i.e.*, $\frac{40}{59}$ or 67.8 per cent of the B momentary reaction potentials (${}_{S}\dot{E}_{R}$) to be greater than the A's. By consulting a suitable table of the normal probability integral it will be found that the 17.8 per cent that 67.8 per cent exceeds 50 per cent corresponds to .462 of the standard deviation (σ) of the normal distribution of the differences here involved. In a word, the reaction potential of B exceeds that of A by $.462\sigma_{diff.}$ By this quantification of the *difference* between two reaction potentials, a major step toward the quantification of the reaction potentials themselves is attained.

13. In case a situation should chance to arise in which there is an analogous quantification of the difference between habit strength A and the absolute zero of habit strength (Z), *i.e.*, just no habit strength at all, the difference between this zero and the value of A would be the habit strength of A;

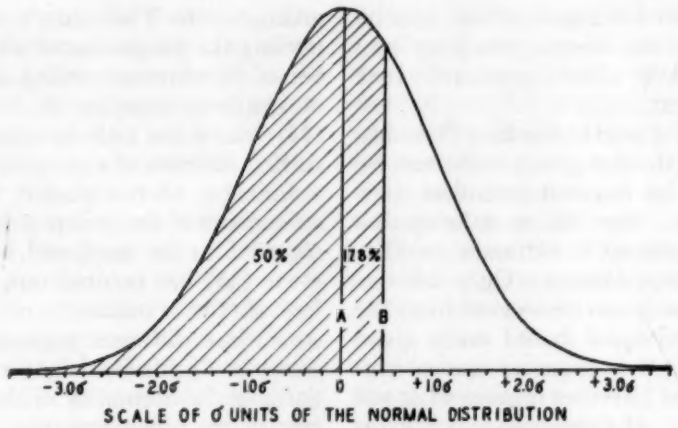

FIG. 1. Diagrammatic representation of the determination of the implied behavioral differences between a habit of two reinforcements (A) and one of three reinforcements (B). The normal distribution represented above is that of the differences between the parallel items of two independent, non-correlated distributions. The σ units should really be marked $\sigma_{diff.}$ to indicate that they refer to a set of differences.

the sum of A and the difference between A and B would be the habit strength of B; and so on for all the other habits in the series. Thus would be finally attained, in an indirect manner it is true, the quantification of a series of habit strengths which it is the objective of the present paper to describe.

14. The close analogy of the procedure so far described to that employed in the methodology of judgmental paired comparisons as employed in traditional psychophysics must have struck the reader. Among the similarities are: the momentary variability in the judgmental response when everything else is constant; the assumption of the Gaussian nature of the variability of these judgmental responses; the per cent excess of one type of judgment over the other; the general method of calculating the difference represented by a given per cent of judgmental difference; and the unit of quantification, the σ of the dispersion of the relevant differences (13, p. 219). Nevertheless, the present data do *not* arise from judgments in any ordinary sense. In psychophysics the judgments ordinarily are based on a comparison of stimulations which are systematically varied, the judgmental response being evoked on the basis of an identical internal condition of relevant habit strength possessed by the judges. In the present situation, on the other hand, the stimulation is in all cases identical while the relevant habit strength which mediates the reaction is the critical difference.

15. Moreover, in psychophysical judgments the two things compared are ordinarily present as stimuli simultaneously or in close succession, whereas the present states under comparison cannot possibly exist simultaneously because only one strength of a given habit can be possessed at a time. This is because when a habit strength is increased the previous strength of that habit no longer exists as such. By the present methodology, the reaction latency is permanently recorded at the time each habit strength exists (even though changed immediately thereafter by reinforcement or non-reinforcement) and the numerical comparison of these records, which may represent states as much as 80

days distant from each other, may be made (by the investigator) at any time whatever after the second event has occurred.

16. Owing also to the fact that only one strength of a given habit can be possessed by a given organism at a given time, there arises a limitation not encountered in ordinary psychophysical experiments. Only one reaction latency can be secured from the reaction potential based on a given habit strength because a response evocation must be either reinforced or not reinforced. If a response evocation is reinforced, it will strengthen the reaction potential; if it is not reinforced, it will weaken it. Therefore the reaction potential can never again be assumed with certainty to be the same as it was. Consequently only one significant comparison can ever be secured from any two habit strengths of a given organism. It thus comes about that in order to secure adequate numbers of comparisons of what correspond to the same habit strength, many different organisms are placed in a comparable learning sequence so that all acquire corresponding habits, it being assumed that the increments in habit strength at the successive reinforcements are in some sense comparable, though hardly equal because of individual differences in learning capacity. For this reason the latencies of a given organism are here only compared with each other, never with those of other organisms.

17. It thus follows that habit strength difference quantifications secured by the present methodology are those between the central tendency of the many habit strengths of the group of organisms as a whole at two different numbers of reinforcements, in terms of the standard deviations of the pooled reaction potential variabilities of the same group of organisms as a whole. In some respects this is analogous to Thorndike's method of pooling the judgments of a large number of persons concerning the quality of concrete samples of handwriting (**32**) where the unit in which the resulting differences were quantified was a function of the pooled variability differences of the group of judges who chanced to be employed. As Thurstone (**33**) has pointed out, this pooling of the variability of numerous somewhat different organisms introduces an additional uncertainty regarding the normality of the distribution of the differences, the σ of which is here used as the preliminary measuring unit of the quantification of reaction potential. Sooner or later, of course, the characteristics of this pooling of many variability distributions must be objectively determined if possible (**11**). Provisionally it is assumed to approximate closely enough to normality to permit useful employment of the usual tables of the probability integral.

THE DETAILED QUANTIFICATIONAL PROCEDURE

A detailed account of the subjects and experimental procedure which yielded the data of the study now being reported was given in the first article of the present series (**7**). It will be necessary here only to say that 59 male albino rats were employed as subjects and that they were trained to move a brass tube to the left a minimum of one-sixteenth of an inch. This tubular manipulandum projected five-eighths of an inch into the animal's chamber and required a lateral pressure of seven and a half grams. The motivation throughout was 22 hours of food privation. A single operation of the manipulandum, always immediately reinforced by a small cylinder of food, was given each animal every day, including week-

ends, throughout the training. The aspect of the reaction directly measured was the time from the lifting of the shutter giving the animal access to the manipulandum until the completion of the animal's reaction. As a sample of the data with which we start, we give in Table 2 the first nine latencies of the first ten of the 59 animals.

There were available for our quantification the experimental data from more than 60 trials of our 59 animals. Such a number of quantifications by the present methodology would involve a prohibitive amount of labor. For this reason a judicious sampling[6] of 25 trials distributed over the entire range of 60 was chosen as the basis for our major investigation (11). But the tables associated with the simultaneous quantification of even 25 variables are very bulky and hardly possible of publication in an ordinary journal. Since, however, the present methodology had not hitherto been applied to the present type of problem, it was felt that a detailed example would materially facilitate the exposition. Accordingly the quantification of the first nine only of the 60 trials was worked out for illustrative purposes, and will be presented here. The outcome of the quantification of the 25 trials will be presented in analytical detail elsewhere (11).

The first step in the methodology of paired comparisons is to compare each of the items under consideration with every other. Such a set of comparisons for our example, that for subject No. 2, is shown in detail in Table 3. In each comparison there is involved the latency of primary reference, the numbered rows of Table 3, and the latency of secondary reference, the numbered columns. The numbers of the rows and columns represent the number of the superthreshold trials during the learning process. In case the latency of primary reference has a shorter duration (which implies a greater reaction potential) than its secondary-reference comparison, a value of unity is placed in the cell marked by the intersection of row and column, *e.g.*, row 2 and column 1. In case the reverse comparison is made, a zero is placed in the cell of opposite combination, *e.g.*, row 1 and column 2. Thus where row and column differ in num-

[6] The trials actually chosen are presented on the abscissa of Fig. 2. Every second trial was chosen early in the series where change is more rapid, and thereafter every third trial was chosen.

TABLE 2

A sample of the original data employed in the present study, the first nine latencies (in seconds) of each of the first ten subjects

Rat No.	Trial numbers								
	1	2	3	4	5	6	7	8	9
1	3.50	4.01	10.03	8.21	25.00	1.00	2.57	2.54	.80
2	27.50	15.41	7.61	18.00	1.43	25.00	6.50	5.82	3.86
3	13.00	9.00	13.50	8.80	20.00	17.00	41.00	10.05	2.00
4	13.00	11.50	5.00	2.00	1.00	1.00	.80	2.00	1.50
5	6.00	4.00	2.80	6.00	8.56	2.27	1.01	1.23	2.07
6	2.50	3.00	1.50	148.00	16.50	25.00	33.50	40.00	22.80
7	5.50	10.05	21.00	9.00	16.00	270.00	4.21	2.56	2.07
8	31.50	39.50	210.00	112.00	34.00	258.00	36.00	53.00	62.50
9	6.20	3.77	8.31	7.26	4.43	16.50	1.81	2.36	1.21
10	49.00	33.00	6.71	27.36	44.00	5.50	3.70	1.51	2.36

TABLE 3

Table of the paired comparisons of the nine reaction latencies of a typical subject, No. 2. Note that each pair of latencies yields two comparison scores because each is used once as the primary reference and once as the secondary reference. The sum of the entries of the columns and the rows of the same number must equal the number of items compared (nine), which makes possible a convenient check of the accuracy of each tabulation.

Ordinal number of latency trials of primary reference	Reaction latencies compared	Ordinal number of latency trials of secondary reference									Check column summating the total of row and column having the same number
		1	2	3	4	5	6	7	8	9	
1	27.50	.5	0	0	0	0	0	0	0	0	0.5+ 8.5= 9.0
2	15.41	1	.5	0	1	0	1	0	0	0	3.5+ 5.5= 9.0
3	7.61	1	1	.5	1	0	1	0	0	0	4.5+ 4.5= 9.0
4	18.00	1	0	0	.5	0	1	0	0	0	2.5+ 6.5= 9.0
5	1.43	1	1	1	1	.5	1	1	1	1	8.5+ 0.5= 9.0
6	25.00	1	0	0	0	0	.5	0	0	0	1.5+ 7.5= 9.0
7	6.50	1	1	1	1	0	1	.5	0	0	5.5+ 3.5= 9.0
8	5.82	1	1	1	1	0	1	1	.5	0	6.5+ 2.5= 9.0
9	3.86	1	1	1	1	0	1	1	1	.5	7.5+ 1.5= 9.0
Σ		8.5	5.5	4.5	6.5	.5	7.5	3.5	2.5	1.5	40.5+40.5=81.0

ber, the comparison score appears twice in the table. In case the two latencies under comparison are the same, a value of .5 is placed in each of the two cells.[7] If a row and column have the same number, only one entry is made, that of .5; hence the diagonal row of .5 values dividing the table into halves. This is in conformity with a psychophysical convention (**13**, p. 226) and represents the fact that the comparison of a value with itself yields a zero difference.

[7] When a value of .5 appears in a table of the type of Table 3, *i.e.*, where the two latencies compared are equal, we have in the present situation what corresponds to an "equal" judgment in ordinary psychophysics. This situation accordingly has substantially the logical status of the "equal" judgments which involve a special relationship to the magnitudes judged (**13**, p. 195). The assignment of a .5 to such cases in effect divides them equally between the two situations being compared. While this probably produced an error in the results, it was slight owing to the small number of such "tied" comparisons. The increased accuracy to be obtained in giving separate treatment to these data was not believed to warrant the added labor in a preliminary determination such as the present one.

The second step in the procedure is to go through all the paired comparison tables, such as Table 3, of the 59 individual animals and find the total of the scores falling in each of the 81 cells. Each one of these totals is then divided by the total number of subjects, here 59. This yields the equivalent per cent of the latencies of each of the pairs compared in which the latency of primary reference is the shorter and therefore the reaction potential the greater. These means are shown in Table 4, corresponding cell for cell to Table 3.

Our third step is to convert the empirical probability values of Table 4 into quantificational separations or differences in units of the standard deviation of the difference variability of the group as a whole, as described above (p. 242). For example, in the first data column of Table 4, it appears in row 2 that 57.6 per cent of the second superthreshold trials of the 59 animals proved to have shorter reaction latencies than the first superthreshold trials. Looking up the cor-

TABLE 4

Means of the several reaction-latency comparison scores of the first nine reactions of the 59 subjects. The values of the cells in this table are the averages of the corresponding cells of the type of Table 3, for the entire group of 59 animals.

Ordinal number of latency trial of primary reference	Ordinal number of latency trials of secondary reference								
	1	2	3	4	5	6	7	8	9
1	.500	.424	.390	.280	.322	.322	.170	.119	.102
2	.576	.500	.322	.288	.322	.305	.170	.153	.119
3	.610	.678	.500	.263	.305	.339	.195	.186	.195
4	.720	.712	.737	.500	.407	.458	.305	.314	.136
5	.678	.678	.695	.593	.500	.517	.348	.356	.305
6	.678	.695	.661	.542	.483	.500	.390	.305	.195
7	.831	.831	.805	.695	.653	.610	.500	.426	.373
8	.881	.848	.814	.686	.644	.695	.576	.500	.331
9	.898	.881	.805	.864	.695	.805	.627	.670	.500

responding σ values in an appropriate table of the probability integral, *e.g.*, Guilford (**13**, p. 537 ff.), we find that this corresponds to .192 $\sigma_{diff.}$ as shown in the same position in Table 5. Similarly, the third set of 59 superthreshold latencies shows 61.0 per cent shorter than the first set, as presented in row 3, column 1 of Table 4, which by the same table corresponds to .280 $\sigma_{diff.}$ as given by the next entry in Table 5; and so on for all the other comparisons with the first superthreshold reaction latency. The 67.8 per cent value and the .462 $\sigma_{diff.}$ value used for illustrative purposes in Fig. 1 appear at the intersection of row 3 and column 2 in Tables 4 and 5 respec-

TABLE 5

Quantificational separations of the nine variables of the group of 59 organisms as a whole in all combinations in terms of the standard deviations of response variability of the variables involved, as found by looking up in a table of the normal probability integral the values corresponding to those of Table 4.

Ordinal number of latency trials of primary reference	Ordinal number of latency trials of secondary reference								
	1	2	3	4	5	6	7	8	9
1	.000	−.192	−.280	−.584	−.462	−.462	−.956	−1.182	−1.272
2	.192	.000	−.462	−.559	−.462	−.510	−.956	−1.026	−1.182
3	.280	.462	.000	−.635	−.510	−.415	−.860	− .891	− .860
4	.584	.559	.635	.000	−.236	−.106	−.510	− .486	−1.100
5	.462	.462	.510	.236	.000	.042	−.392	− .369	− .510
6	.462	.510	.415	.106	−.042	.000	−.280	− .510	− .860
7	.956	.956	.860	.510	.392	.280	.000	− .192	− .324
8	1.182	1.026	.891	.486	.369	.510	.192	.000	− .439
9	1.272	1.182	.860	1.100	.510	.860	.324	.439	.000
Means	.5989	.5517	.3810	.0733	.0490	.0221	−.3820	− .4686	− .7274
Means squared	.3587	.3044	.1452	.0054	.0024	.0005	.1495	.2196	.5291

tively. In a similar manner were obtained from the proportion or probability values of Table 4 all the other quantitative behavioral differences shown in Table 5.

At this point we introduce into our quantificational methodology a unique procedure devised by Thurstone (**33**, **34**)—the so-called Case III of his paired-comparisons methodology. This yields an estimate of the dispersion of each of the individual variables involved (σ_k) and mediates an important rectification of the scale-difference values as secured from the table of the probability integral. The procedure of securing the σ_k's consists of several stages, the results of which are summarized in Table 6. It begins with the calculation of the standard deviation of each column of Table 5. To do this each item in each column of Table 5 is squared and the squares are added and divided by the number of items; this gives the mean of the X^2's, *i.e.*, $\frac{\Sigma X^2}{N}$. Then the squares of the means of the columns of Table 5 are transferred and placed in the formula

$$V_k = \sqrt{\frac{\Sigma X^2}{N} - M^2} \qquad (1)$$

in Guilford's notation. For example, by this formula the standard deviation of column 1, Table 5, becomes

$$V_k = \sqrt{\frac{4.8122}{9} - .3587}$$

$$= \sqrt{.5347 - .3587},$$

$$V_k = .4195.$$

These V_k values for the present data are given as the first row of Table 6.

The next stage of the process is to find the reciprocal of each V_k value, *e.g.*,

$$\frac{1}{.4195} = 2.384.$$

These appear as the second row of values in Table 6. All the nine $\frac{1}{V_k}$ values are then added, which comes to 20.211, as shown in the Σ column at the right. With the $\Sigma \frac{1}{V_k}$ values available the value, a, may be calculated by the formula

$$a = \frac{2n}{\Sigma \frac{1}{V_k}}. \qquad (2)$$

Substituting in our example,

$$a = \frac{2 \times 9}{20.211};$$

$$\therefore \quad a = .8906.$$

We now have the values necessary for finding the σ_k's which are our immediate objectives. These are given by the formula

$$\sigma_k = a \frac{1}{V_k} - 1. \qquad (3)$$

Substituting, we have as σ_k of variable 1

$$\sigma_k = \frac{.8906}{.4195} - 1$$

$$= 2.123 - 1;$$

$$\therefore \quad \sigma_k = 1.123.$$

The values of the σ_k's as so calculated appear in the third line of Table 6.

Now, the ultimate object of calculating the σ_k's as above is to enable us to rectify certain distortions in the scale-difference values as they come from the probability table as shown in Table 5. This arises from the facts (1) that these latter values are in terms of $\sigma_{diff.}$, and (2) that these $\sigma_{diff.}$'s as units of quantification are tacitly assumed to be equal. Actually they are not equal, as is shown by implication in the third line of Table 6. To understand this we need to recall

TABLE 6

Stages in the calculation of the σ_k's which are the estimates of the dispersions of the several individual variables. It will be noticed that the smaller the V_k, the greater the σ_k.

Symbolic representation of the several computational stages	Ordinal number of the several variables									
	1	2	3	4	5	6	7	8	9	Σ
V_k	.4195	.4295	.4823	.5559	.3760	.4387	.4580	.4864	.4084	
$\frac{1}{V_k}$	2.384	2.328	2.073	1.799	2.660	2.279	2.183	2.056	2.449	20.211
σ_k	1.123	1.073	.846	.602	1.369	1.030	.944	.831	1.181	8.999
σ_k^2	1.261	1.151	.716	.362	1.874	1.061	.891	.691	1.395	9.402

a fundamental equation used by Thurstone (33, p. 280),

$$S_2 - S_1 = X_{21} \cdot \sqrt{\sigma_2^2 + \sigma_1^2} \quad (4)$$

in which

$S_2 - S_1$ represents the difference between any pair of quantified reaction potentials used as variables;

X_{21} = the sigma value of the difference between 1 and 2 as derived from Table 4 and appearing in Table 5 as described above;

σ_2 = the dispersion of variable No. 2;

σ_1 = the dispersion of variable No. 1.

The σ_k's are given in Table 6, *e.g.*, 1.123 and 1.073. Substituting in equation (4), we have

$$\begin{aligned} S_2 - S_1 &= .192\sqrt{1.261 + 1.151} \\ &= .192 \times 1.553 \\ &= .298. \end{aligned}$$

Carrying out similar statistical maneuvers for all of the other combinations we have the values of Table 7,

TABLE 7

Quantificational separations of the nine variables of the group of 59 subjects as a whole as derived from Tables 5 and 6, with the aid of Formula 4.

Ordinal number of latency trials of primary reference	Ordinal number of latency trials of secondary reference								
	1	2	3	4	5	6	7	8	9
1	.000	−.298	−.394	−.744	−.818	−.704	−1.402	−1.651	− 2.073
2	.298	.000	−.631	−.688	−.803	−.758	−1.366	−1.392	− 1.886
3	.394	.631	.000	−.659	−.821	−.553	−1.090	−1.057	− 1.250
4	.744	.688	.659	.000	−.353	−.126	− .571	− .499	− 1.459
5	.818	.803	.821	.353	.000	.072	− .652	− .591	− .922
6	.704	.758	.553	.126	−.072	.000	− .391	− .675	− 1.348
7	1.402	1.366	1.090	.571	.652	.391	.000	− .242	− .490
8	1.651	1.392	1.057	.499	.591	.675	.242	.000	− .634
9	2.073	1.886	1.250	1.459	.922	1.348	.490	.634	.000
Σ	8.084	7.226	4.405	.917	−.702	.345	−4.740	−5.473	−10.062

the first item in row 2 being the .298 calculated above.

It may next be noticed that in Table 7 we have eight distinct measures of the scale difference between the members of each successive pair of reaction potentials. If we let S with a subscript corresponding to the number of the superthreshold trial (including the first) represent the nine reaction potentials here under consideration, it is evident (assuming a normal distribution and a very large sample of data) that the value of $(S_3 - S_1)$ less the value of $(S_3 - S_2)$ must represent the value of $(S_2 - S_1)$, and so on. Stated formally and systematically,

$$(S_3-S_1)-(S_3-S_2)=S_2-S_1=d_{12}$$
$$(S_4-S_1)-(S_4-S_2)=S_2-S_1=d_{12}$$
$$(S_5-S_1)-(S_5-S_2)=S_2-S_1=d_{12}$$
$$\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots$$
$$(S_9-S_1)-(S_9-S_2)=S_2-S_1=d_{12}.$$

These seven values together with that secured directly by subtracting S_1 from S_2 give in all eight values for d_{12}, or one less than the total number of the variables involved. Actually, since $(S_3 - S_1) \cdots (S_9 - S_1)$ constitutes column 1 of Table 7 and $(S_3 - S_2) \cdots (S_9 - S_2)$ constitutes column 2, this series of mathematical expressions amounts to a mere subtraction of the entries of column 2 of Table 7 from the corresponding values of column 1. These differences are recorded appropriately in column 1 of Table 8. In a similar manner the subtraction of column 3 from column 2 yields the eight values of d_{23}, the subtraction of column 4 from column 3 yields the eight values of d_{34}, and so on.

Now, the mean of each column of scale-difference values will in general yield a more reliable measure of the quantificational differences in question than will any single difference value. The eight versions of the quantitative difference between each of the successive variables are accordingly averaged and appear as the next-to-last line of Table 8. These represent the culmination of our

TABLE 8

Eight different evaluations of each of the eight quantificational differences between the reaction potentials separating each successive pair of trials, together with their means and cumulations.

Ordinal number of trials	The eight types of differences involved							
	d_{12}	d_{23}	d_{34}	d_{45}	d_{56}	d_{67}	d_{78}	d_{89}
1		.096	.350	.074	−.114	.698	.249	.422
2	.298		.057	.115	−.045	.608	.026	.494
3	−.237	.631		.162	−.268	.537	−.033	.193
4	.056	.029	.659		−.227	.445	−.072	.960
5	.015	−.018	.468	.353		.724	−.061	.331
6	−.054	.205	.427	.198	−.072		.284	.673
7	.036	.276	.519	−.081	.261	.391		.248
8	.259	.335	.558	−.092	−.084	.433	.242	
9	.187	.636	−.209	.537	−.426	.858	−.144	.634
Σ of $_sE_R$ differences	.560	2.190	2.829	1.266	−.975	4.694	.491	3.955
M of $_sE_R$ differences	.0700	.2738	.3536	.1583	−.1219	.5868	.0614	.4944
Cumulative $_sE_R$ values	.0700	.3438	.6974	.8557	.7338	1.3206	1.3820	1.8764

quantifications of reactional-potential differences.

There remains, however, the task of finding the relative magnitude of the several reaction potentials from the lowest available reference point, which is arbitrarily taken as zero. This naturally is the reaction potential of the first trial during the learning process showing a conditioned reaction, *i.e.*, a minimum superthreshold reaction potential. This means that reaction No. 1 has a value of zero; No. 2 has a reaction potential represented by the difference between 1 and 2, *i.e.*, d_{12}, which is .07; the reaction potential of No. 3 is at that distance above zero which is the sum of $d_{12} + d_{23}$; and so on for the other values up to trial No. 9, which has a value of 1.8764. These values, consisting of the cumulation of successive differences, are shown in the last line of Table 8.

Thus reaction-potential differences as generated by a simple learning process have been quantified and, in some sense, measured (**4, 3, 15, 23, 26**). The quantificational unit employed is the joint standard deviation of the oscillatory tendencies of the 59 organisms employed as subjects on the trials quantified. By the same token the present description of the operations involved in this quantification constitutes a detailed operational definition of reaction potential ($_sE_R$) itself.

THE VARIATION IN σ_k AS LEARNING PROGRESSES

The central concept under examination in the present section is Thurstone's 'confusion range' (**33**, p. 285), which is said by him to 'fluctuate' (**33**, p. 274). This fluctuation of judgmental reactions is evidently related

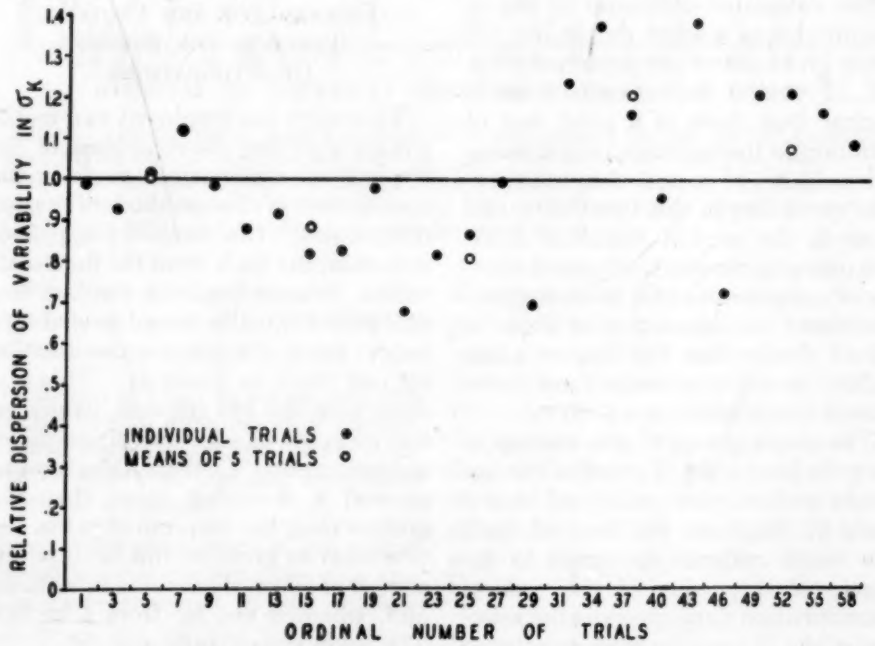

FIG. 2. A graphic representation of the relative dispersion estimates (σ_k) of the variables used as the basis for measuring the respective reaction potentials at 25 of the 60 learning trials selected for the purpose of quantification.

functionally to what the senior author of the present article has called the 'oscillation' ($_{S}O_{R}$) of reaction potential (**20**, p. 304). The question of primary interest now before us is whether, except for the influence of sampling, the dispersion range of the individual variable (σ_k) varies systematically as the learning process progresses. Fortunately, as we have seen, Thurstone has provided us with a procedure for estimating the relative dispersion of the several elements entering into paired comparisons with every other in a quantificational situation such as the present. This is an approximation based on the assumption that the actual differences in dispersion are not very great; *e.g.*, that (**33**, p. 286) the dispersion of one of the compared elements (No. 2) will not be more than about twice that of another (No. 1) as a maximum difference.

The calculated estimates of the σ_k magnitudes as worked out in our example given above are presented on p. 247. From an inspection of these it is clear that there is a good deal of variation in the estimated dispersions. It is evident, of course, that considerable variability in this function would occur in the present empirical situation owing to the relatively small number of comparisons (59) in each quantification. An inspection of these σ_k values shows that the largest value (1.369) is slightly more than twice that of the smallest one (.602).

The above group of σ_k's, coming as they do from a set of genuine but exemplary data, are presented essentially to illustrate the methodology. Our main evidence in regard to the degree of dispersion of the present quantification data concerns the selection of the 25 learning trials mentioned earlier (p. 243). In order to facilitate the reader's comprehension of this rather complex set of 25 σ_k's, they are presented graphically in Fig. 2. There it may be seen that just as in our sample of nine, the variability from trial to trial is considerable. The largest value (1.381) is also slightly more than twice that of the smallest (.678).

An inspection of Fig. 2 shows in general a tendency of the σ_k's to fall to a minimum near $N = 25$, after which they rise. To reduce the confusing effect of random variability apparently due largely to sampling limitations, the σ_k's were averaged by successive 5's. The results are represented in Figure 2 by hollow circles. These mean values show a progressive fall from near the mean value at trial 5 to a minimum at trial 25, after which the σ_k's rise markedly to relatively high levels throughout the final half of the learning.

THE APPLICATION OF THURSTONE'S DISCREPANCY AND PLOTTING TESTS TO THE PRESENT QUANTIFICATIONS

Thurstone has employed two methods for verifying the soundness of the dispersional assumptions made in the application of his method of paired comparison. One method (**35**, p. 296) is to compute back from the final scale values secured (such as the last line of Table 8) to the actual probability values from which the quantification set out (such as Table 4). This was done with the 288 relevant values involved in the 25 variables of our major quantification. Of these a single value showed a deviation more than 3σ greater than the theoretical value. A deviation as great as this is to be expected in about three cases in a thousand, which is not far from 1 in 288. The mean discrepancy was .025.

A second method employed by Thurstone to check on the dispersions involved in a given quantification (**34**,

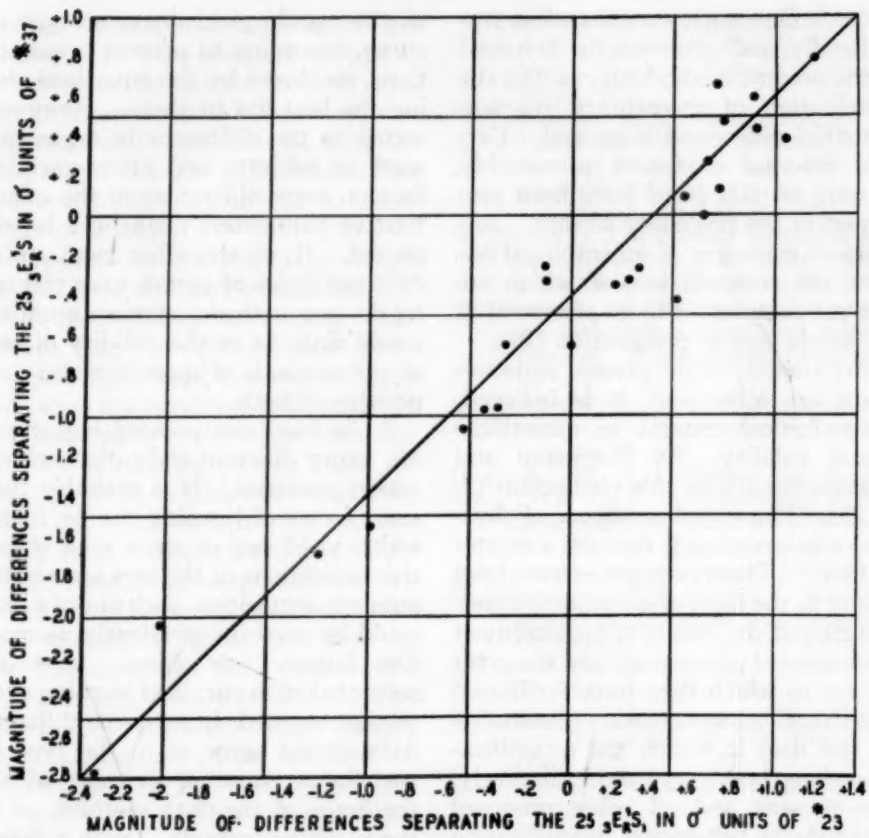

FIG. 3. Dot diagram of column having trial 37 as a common variable, plotted against the column having trial 23 as a common variable. It will be seen that these dots, despite considerable scatter, approximate a linear sequence with a slope of unity. The diagonal line drawn through the dots has a slope of exactly 1.00. The linearity as represented by zeta (13, p. 348) is .0371, and the calculated slope deviates from unity by .014.

p. 389) is to plot against each other any two columns of such tables as Tables 5 or 7 above. He states:

"If the plot is linear it proves that the assumed normal distribution of the discriminal process is correct. If the slope is unity, it proves that the discriminal dispersions are equal."

Fifteen pairs of columns of our major quantificational project involving the 25 trials represented in Fig. 2 were thus tried out on the tables corresponding to Tables 5 and 7. It was found in general that the plots coming from the tables corresponding to Table 5 (representing Case V) were approximately linear, but their slopes deviated perceptibly from unity. However, when the same plots were made from the table corresponding to Table 7 (representing the employment of Case III), their calculated slopes were close to unity, their deviations averaging only .0248. A fairly typical plot of this type is reproduced as Fig. 3.

DISCUSSION

The scientific validity of a method of measurement or quantification is dependent in part on *a priori* consider-

ations. One such consideration traditionally and prominently involved in the present methodology is that the distribution of momentary reaction potential differences is normal. Certain internal evidences presumably bearing on this point have been presented in the preceding section. Additional evidences of an empirical nature, the computations of which are not yet complete, will be presented in an article now in preparation (**39**).

Fortunately in the present situation there are other and, it is believed, more critical criteria of quantificational validity. As Bergmann and Spence remark in this connection (**3**, p. 23), "The actual excellence of physical measurement is entirely a matter of fact." These excellences have been found in the facts of quantitative consistency of the results of measurement with related phenomena and the great extent to which they have facilitated the simplified systematic organization of the field in which the quantifications have been employed. Similarly, the present and all other proposed methods of behavioral quantification must depend for their validation, at least in part, upon the quantitative consistencies and systematizations to which they give rise. One or two specific examples of such systematic possibilities in the present field may clarify and give point to this statement.

1. It is evident from the description of the present experiment and associated quantificational methodology that a primitive curve of learning should emerge. Now, a closely related method based on reaction thresholds in the case of conditioned winking of the type reported by Reynolds (**27**) should also yield a learning curve according to a published theoretical analysis (**20**, p. 327). With a suitable adaptation of Reynolds' experimental procedure and with more subjects available, the two methodologies should yield the same type of curve, according to present presumptions, as shown by the equations giving the best fits to them. However, owing to the difference in organisms used as subjects and other variable factors, some differences in the quantitative parameters might well be expected. If, on the other hand, quite different types of curves were yielded by the two methods, a serious question would arise as to the validity of one of the methods of quantification and possibly of both.

2. As has been pointed out, there are many different indications of reaction potential. It is probable that some forms of learning can be found which yield two or more such objective indications of the *very same* habit strength acquisition, each one of which could be used independently as reaction latency was above. It would seem that such curves of learning even though secured from quite different data should agree as to the type of best fitting equation and also, within the limits of the data sampled, as to the various constants. Such a determination may very well prove to be a critical test of the methodology.

Concrete illustrations of the scientific fertility and consistency tests of the present quantificational methodology will be presented in two other studies (**11**, **39**). However, no single test or group of tests can completely effect such a validation. This must be left largely to the future.

SUMMARY

There is reason to believe that most, if not all, behavior is quantitatively determined by two types of internal conditions which mediate it: habit strength ($_sH_R$) and drive (D) which presumably combine in a multiplicative manner to produce reaction potential ($_sE_R$). In spite of this pre-

sumptively common basis of all behavior, there is no common method of measurement or quantification of ${}_sE_R$, and no common unit. This may very well account in considerable part for the present relative lack of quantitative systematization in the so-called behavior sciences.

It is probable that the major reason for the lack of a uniform quantificational methodology in the very numerous behavior studies is that reaction potential, both as habit strength and drive, is a condition hidden within the behaving organism, the existence of which is indicated, though only indirectly, by signs which are quantitatively about as various as is the behavior itself. One hopeful possibility is that while all these quantitative indicators bear different functional relations to reaction potential, it is probable that all, or nearly all, of them are monotonically related to it. On this assumption, together with certain other assumptions which are more or less common to the various psychophysical and psychological scaling methods, a sample quantification has been presented in detail for the first ten trials of a habit formation sequence. This methodology is believed to constitute a strict operational definition of ${}_sE_R$.

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TEN BASIC POSTULATES OF PERSONALISTIC PSYCHOLOGY

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Introductory

It will be noted that the writer is using the terms 'personalistic psychology' as synonymous with 'psychology of personality.' This identification will not be pleasing to some but the writer, with apologies to the late William Stern, believes that there is justification for the usage. In a previous article (14) the writer used the terms synonymously but, in his preoccupation with the main thesis of his article, forgot to inform the reader of the fact. G. W. Allport took to task the writer in a reply (3) to that article. Allport's criticism, under the circumstances, was quite justified. However, it was not a case of an 'inaccurate understanding of personalistic psychology,' as Allport said, that led the writer to use the term 'personalistic psychology' as a generic term covering strictly personality psychology. So, once more, for better or for worse, we will use the term personalistic psychology to stand for all forms of what we regard as legitimate personality study.

William Stern introduced the term 'personalistic psychology,' using the term in the title of his book, *General Psychology from a Personalistic Standpoint* (16). True enough, as Allport says, ". . . Stern's writing is as nomothetic as one's heart could possibly desire." However, Stern's psychology includes several of the basic postulates of any legitimate personality psychology. The term, *personalistic*, used as an adjective, refers to the individual person and stresses uniqueness. Stern defines personalistic psychology as ". . . the science of the *person* having experience or capable of having experience." Again, "experience is therefore to be identified and interpreted in terms of its matrix, the unitary goal-directed *person*" (16, pages 7 and 8 of the preface). And on another page Stern says "the person is a living whole, *individual, unique*, striving toward goals, self-contained and yet open to the world around him; he is capable of having experience" (16, Ch. IV, p. 70). We wish to call attention to the terms *individual* and *unique*. These are basic conditions for any psychology of personality and these terms justify us, we believe, in using the term *personalistic* in a new and modified way. Even Allport's book, *Personality* (1), is saturated with the Stern tradition and Allport, in his criticism of my previous paper (3), says that ". . . each person is a unique individual regardless of who defines what how." However, we wish to insist, as did Stern, that there are many things that are *not unique* to each individual but common to all men. When we study these *common-to-all-men* items we have *scientific content* and a true *generalized mind*. The point is that Stern could write a nomothetic psychology, because his system admitted *common elements or data in all men*. When he talked of truly unique things he was talking strictly in terms of a psychology of personality! So Stern's work included both a psychology of personality and a general psychology.

Personalistic Psychology Today

It has seemed worth while to the writer to attempt to present certain basic postulates which seem to be rather

generally stated or implied in the teachings of those who are chiefly concerned with the psychology of personality. The field of study has grown remarkably, both in theory and in experimental observation and fact. With due respect to the contributions of such men as Freud, Adler, Jung, McDougall, William Stern, and many others, the writer would select G. W. Allport, K. Lewin and H. A. Murray as outstanding contributors to basic theory today (1; 7, 8; 10, 11). Rosenzweig has pointed out that these men seem, on the surface, to be far apart but that more careful study indicates a gradual converging of their ideas (12). Allport operates in the tradition of the personalistic background of William Stern, Murray is intrigued with psychoanalytic doctrines, and Lewin thinks in terms of the Gestalt school.

The movement toward personality study is vigorous and aggressive today. Beginning with the basic notion that general psychology has signally failed to give the students thereof a real grasp and understanding of human nature, many insist that we find something better than the older experimental psychology. This 'something better,' they insist, is the study of personality. Thus Allport says, "It has, for example, often been pointed out that the psychologist, in spite of his profession, is not a superior judge of people" (1, page 5). This is a strong, clear statement which is subscribed to by many students of human nature. At Wayne University the directors of the College of Education have recently concluded that it is largely a waste of time for their students to take beginning psychology in the College of Liberal Arts! So this course is no longer a prerequisite for the students who are going into education! Rather they have substituted a course of their own which they call a course in 'Human Relations.' These things are signs of the times! There are those who would substitute a course in personality for the usual beginning course in psychology. One gains the impression, in some quarters, that the psychology of personality is the complete answer to all of the psychologist's prayers! Certainly it behooves us to inspect closely the values and limitations of this newer emphasis in psychology. We believe that there are ten basic postulates which characterize the general movement in personalistic psychology. These we will now state and briefly discuss some of their implications.

Postulate I. If all persons were exactly alike in every psychological characteristic, would we speak of personality? Most personalistic psychologists would probably answer no. We may be interested in how people are alike or in how they differ. But when we consider how people differ we find that they differ in the degree or amount of possession of any given capacity, trait or characteristic. Here the data are common to all persons of the group and the differences are merely in degree. On the other hand, we may find differences *in kind*, not just in degree. We may find that John possesses some elemental characteristic or some combination, pattern or Gestalt of characteristics which no other living organism possesses. The tendency today is for students of personality to stress these individual characteristics, in a word, *uniqueness*. One's personality consists of those characteristics which are unique. Tom Jones alone in all the world has them! This is why the writer, in an earlier article (14), indicated that the study is ideographic and cannot be scientific *in content* in the ordinary scientific sense.

Allport stresses this fact of uniqueness but finds that *certain characteristics are also common to the group.* So he speaks of *common traits* and

individual traits. According to the postulate which we will now formulate, Allport's common traits are not strictly a part of personality, because they are common property to all.

The first postulate then would state that, *in a strict sense, personality refers to the organization of characteristics which are unique or particular to a given individual and to no other.*

Many would object to such a restricted definition of personality, no doubt. However, we believe that theoretical consistency forces us to accept the postulate. The question at once arises as to whether we can have a science of personality? As the writer pointed out in a previous article, the answer is no, if we refer to the content, as such, of the study. Generalization depends upon the detection of common elements or similarities in observed data. There are no common elements or similarities in *unique* data (14)!

In terms of methods and in terms of the conditions which produce the unique phenomena, personalistic psychology has adequate claims to being scientific. When Allport, Stagner, or Thorpe (1, 15, 17) write a textbook on personality, what do they do? They do not write a detailed case-study of some one person. They concern themselves with general or common conditions; they study the common basic processes of all organisms and the environmental conditions, which, in some way, produce these unique data. General laws of growth and development of personality may be found to the extent that they are based on *common factors* which produce the unique characteristics! A strictly personalistic psychology, in terms of content of unique patterns, would be an individual case-study of some one person, however.

Postulate II. The early behaviorists of the Watson school at least implied that, if we had complete knowledge of the stimulating environment and complete knowledge of the muscular and glandular responses, we would have a complete system of psychology. The stimulus was certainly placed on a 'throne.' The personalistic psychologist today has, if anything, a tendency to go to the other extreme. Stern stressed the internal psycho-neural organization as *dynamic and motivational.* It stood between stimulus and response. Now part, at least, of this out-of-sight, internal, psycho-neural organization *is the personality.* Allport has developed this thesis and made it a pillar of his theory of personality.

The second postulate may then be stated as follows: *Personality, whatever be its basic nature, lies within the individual and functions as an organization between stimulus and response. It is not open to observation. It is known only through inference.*

Postulate III. The preceding discussion has prepared us for the third postulate. It goes as follows: *The inner psycho-neural organization which is the personality, is dynamic and motivational. As it gains in extent of organization it functions more and more to interpret and select stimuli, on one hand, and to control behavior on the other.*

The implications of this postulate have been so adequately stated by Allport that little more will be said here. As Allport says, this postulate "dethrones the stimulus." Whatever be the nature of these processes in this internal psycho-neural organization, they are truly dynamic and motivational. Man is not just a creature at the mercy of every passing stimulus! The personality "goes places and does things on its own!"

On the other hand there is much danger here in going too far in making the personality independent of the stimulating situation. In a world of

causes and effects it is dangerous to neglect any item in a chain of such causes and effects. There are many times and occasions when the situation seems to be the determiner of our decisions, choices and acts! Also, there is the very respectable theory in psychology which holds that, if all stimuli were removed and no afferent impulses were delivered over the threshold of the central nervous system, all consciousness and mental activity would cease. One needs to keep a good balance between the stimulus and the personality as respective conditioning factors.

Postulate IV. A fourth postulate, emphasized by all students of personality psychology, may be stated as follows: *Personality refers to an organization, a total pattern. No one part or aspect of this total pattern may be called the personality.*

It may be noted that *the total psychological man is much more than the personality.* Personality is, then, not to be confused with 'the whole man active.' Personality is not the sum total of all psychological processes and characteristics; it is much less than that.

However, the term personality must stand for a total, large pattern of integration. It is not correct to pick out some aspect, such as the emotional or intellectual aspect, and call it the *personality.* A 'personality test' and a 'test for some aspect of personality' are not at all the same. Just how one would go about devising a 'test of personality' is beyond the knowledge of the writer. Just how one could ever express in any quantitative terms the description of the total, complicated pattern which we call personality is beyond the grasp of the writer. One may well raise the question whether it is humanly possible to conceive of or understand any *total personality.* At best one seems able only to explore and understand some few aspects or parts of any personality. Some of the more enthusiastic explorers in the field of personality give one the impression that they are really getting at the whole personality. Especially do some of the Rorschach enthusiasts and the psychoanalysts tend to give this impression. The truly experimental-minded person realizes that at best he can get at only some fragment or aspect of the personality. Instead of talking about a 'test of personality' it would be more sensible to speak of a 'test of some part or aspect of personality'!

Another danger of the fourth postulate is the tendency that it encourages in students to become vague and qualitative in their descriptions of a personality. This neglect of quantitative method is seen in the writings of European psychologists, especially the Vienna products. Some of our educational psychologists seem disposed to go over to the side of the qualitative, literary and intuitional ranks! Since it is impossible to do quantitative work on the *total personality,* this easily encourages a trend away from quantitative work. The book, *The Adolescent Personality* (5), written by Blos and his co-workers, is a good example of qualitative psychology gone wild. Fortunately there are now signs that the Rorschach tests may become more quantitative! The psychoanalysts, however, still seem to abhor measurement.

Postulate V. The fifth postulate demands that the personality be considered as a growing and developing thing. We may state it as follows: *Whatever be the ultimate nature of the psychoneural organization which we call personality, it grows and develops during the life-span of the individual. The growth is from more simple and unorganized parts to a more complicated organized whole.*

One may draw an analogy with the construction of a house. Day by day

the carpenters build the house out of the basic materials at hand. Finally comes the day when the structure is complete. The personality also undergoes a process of construction and organization. We note that the child of one year has a very simple and undeveloped personality. At twenty the personality organization is much more complex and significant. Like the construction of the house, which will appear very different at different times in the course of its construction, so the personality will be different at different times in the life-span.

Postulate VI. The sixth postulate states that *growth and development of personality from the beginning depends upon two basic processes, namely, differentiation and integration. Both of these two processes, in turn, are aspects of two basic processes of all growth, namely, maturation and learning.* These two processes of integration and differentiation are not at all antagonistic but rather are supplementary. Allport (1) has given an excellent treatment of these processes and our only criticism would be that he should make maturation and learning the basic processes, each involving differentiation and integration. His list of 'Aspects of Growth' (1, page 132) seems to the writer to be dependent upon the four basic processes mentioned above. Unfortunately we understand but little of the basic bio-chemical processes which give rise to learning and maturation.

The problem of integration is probably essentially the old problem of association. If we knew what really happened in any conditioned reflex or in the formation of any association between sensory and ideational content, we would probably know the basic principles of explanation for integration or organization. The writer cannot throw out of court the older notions of identical elements. If we wish to talk about similarities and equivalents, as some do, in place of identical elements, one may well heed the recent statement by Boring (4) that we "must admit that identity lies at the end of a series of increasing similarities."

Postulate VII. The seventh postulate states simply that whatever is built up into a structure can also be torn down or cast asunder. More formally stated the postulate is as follows: *Personality, at any stage of its development or organization, may become disorganized, dissociated, or disintegrated. This disorganization may be of any degree of extent or duration.*

Again using the building analogy, we note that when the building is completed, it may at once be torn apart and the parts scattered to the four winds. Or, at any stage of construction of the building, the structure may be torn down. In psychology the distinction between structural and functional dissociation has been of some value, although all functional dissociation probably in some way involves some structural changes in the neurones and synapses. Generally the so-called structural disintegrations, such as are found in paresis, alcoholism, old age dementia and tumorous growth in the brain, have been considered to be more serious than the functional disorders. Cases of so-called functional dissociation, such as are found in the various forms of hysteria, while severe enough while they last, have usually been but temporary conditions and not regarded as serious as the structural cases.

Since the cases of functional dissociation are the ones which are usually considered by the psychologist especially to be in his field of study, such cases are usually implied when we speak of disorganized or disintegrated personality. Again our ignorance of the neurological or bio-chemical processes that are involved is profound. If we knew

what really takes place in the nervous system when a conditioned reflex is extinguished or any desired associative item is forgotten and cannot be recalled, we would probably have the basic explanation of the larger processes of personality disorganization!

Postulate VIII. Assuming that the basic materials for maturation and all learning are furnished by heredity, the eighth postulate states *that personality is chiefly moulded or formed by environmental factors, especially the social factors.* Most writers in the field of personality today lay great stress upon the kind of environment in which the person developed—his home life, kind of parents, education, training, etc. Thus aggressiveness, as a personality trait, would be largely the product of the special kind of education, training and experiences one has had. So the many other traits, such as generosity, humor, conservatism, honesty, introversion, *et al.*, are acquired and not inborn. However, some consider certain traits to be largely the products of hereditary predisposition, such as introversion and emotionality.

Allport (1, p. 107) says explicitly that "the newborn infant lacks personality." Later he says that while personality is not formed at birth it may be said to begin at birth. According to this point of view, the time of birth is the critical time. Before birth the infant has no personality although it does manifest individuality. Hence *personality cannot be equated with individuality* psychologically. Individuality will be personality only if said individuality consists of adjustive reactions which are formed in meeting the environment in which the child must live after birth. Others would not agree with Allport in setting such a sharp line of demarcation between no-personality and the first rudimentary personality. For all practical purposes the writer would agree with Allport but, in theory, he would object to the arbitrary starting point. It is like asking when consciousness first appears, or when the first nerve-muscle reaction occurs. Such studies as those by Gesell and other embryologists indicate that growth and development is a forward-moving continuous process in which stages are mere arbitrary divisions superimposed by man (6). The writer would prefer to say that any and all forms of learning that take place in the uterine condition should be accepted as a part of personality as long as they are distinctive or individualistic.

Some students in the field of personality stress the deeper inherited aspects of a personality. The psychoanalysts, postulating some one or more basic (common) instincts, certainly stress the inherited aspects of personality. Likewise the Rorschacher is likely to talk of the natural, instinctive part of the person. In these cases these students do not agree with Postulate I, as we have stated it.

Postulate IX. The ninth postulate states that *personality is a part of general psychology but represents the most developed and significant aspect of all mental life or organization in the human adult.* In a word, all personality study is psychological study but not all psychological study is personality study. If one never developed beyond the stage of reflexes and instincts, one would never reach the higher level of personality; there would be no personality. If one studies the reflexes for and in themselves, one is not studying personality *per se.* If one developed beyond the stage of reflexes but ended with merely a million-and-one independent, specific stimulus-response processes (habits) one would still not have attained a personality. So one may study psychology on a *non-personality level* or on a *personality level,* depending on

whether one gets at the 'higher' units of life, such as the attitudes, the interests, the moods, the purposes, the sentiments, and the traits (13). Obviously the personality organization plays an increasingly more important role in life as it grows from infancy to adulthood and maturity.

Postulate X. A tenth and final basic postulate is as follows: *The total pattern of organization which we call personality demands some unifying principle.* One of the chief difficulties of conceiving the personality as a unity or an 'existence in itself' lies in the fact that personality, like all psychological concepts, is a *functional* concept. It is not so difficult to look upon a block of concrete and conceive of it as a unit-structure. Made up of millions of particles of sand and cement it holds together and has a boundary line that marks it off from the surrounding world of objects. It is likewise easy to think of the human body-structure as a unitary, separate thing. But when he tries to conceive of functional phenomena, such as 'a personality,' the writer must confess that he is not comfortable. Yet we continue to speak of this organization of part-processes as 'the personality.'

If we must continue to think of any personality as a unitary, functional organization, and not in terms of a physical structure of neurones and synapses, it is evident that some unifying principle or principles must be invoked. Again Allport has probably met this problem more adequately than anyone else (1) as the writer has commented upon in an earlier paper (13).

There is a tendency today to conceive of something *within* the personality which has these unifying qualities. The Soul of religion is such an entity. In philosophical and psychological writings one comes across a variety of little 'controlling Gods.' The Ego, in any one of its numerous meanings (see 2), has been thought of as the guiding and regulating principle (entity?). Sometimes it is a 'cardinal trait' or a 'master sentiment of self regard' (9, Ch. IX). The danger here lies, of course, in making some important part of the personality organization the essential coordinator and unifier when it is only a part-process. A greater danger lies in creating some mysterious, high-sounding entity such as the Soul, the Ego, or the Id. Again we may raise the question as to whether 'the personality' or 'a personality' is more than a figure of speech; or, at most, a convenient verbal label to refer to the many higher organizations of sentiments, attitudes, and traits.

SUMMARY AND CONCLUSION

We have stated what appears to us to be the ten present-day basic postulates of personalistic psychology or the psychology of personality. Their implications have been briefly discussed. Probably the greatest disagreement would be in conection with Postulate I. It makes a world of difference whether personality is defined in terms of parts or configurations which are common to the group or whether it is defined in terms of strictly individual unique characteristics. Obviously Postulate I is a very restricted definition of personality. The content of study, the writer still believes, is strictly ideographic if the definition is in terms of uniqueness.

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ACQUISITION OF MOTOR SKILL: I. QUANTITATIVE ANALYSIS AND THEORETICAL FORMULATION [1]

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INTRODUCTION

For some time it has been thought that the frequently found advantage of spaced practice and the phenomena of reminiscence are somehow related. McGeoch's discussion (11) of reminiscence and spaced practice indicates that at least the following 'explanations' have been considered common to both fields: rehearsal, fatigue effects, perseveration, motivational changes, and differential rates of forgetting. Certain theoreticians have attempted to formulate this relationship more precisely, *e.g.*, Hull, *et al.*, in the *Mathematico-Deductive Theory of Rote Learning* (7), and Hull in *Principles of Behavior* (8).

It is the purpose of this paper to lay a partial foundation for a theory of motor learning directed primarily toward handling reminiscence and spaced practice phenomena within a single system. To begin with, several motor learning curves will be presented and certain characteristics of such curves pointed out. The next step will be an analysis of these characteristics of motor performance curves in terms of certain relatively specific theoretical formulations proposed elsewhere in the literature. On this basis a set of variables will be proposed, and a tentative set of assumptions and deductions set up. Empirical evidence will be cited where possible.

'SIGNIFICANT' PHENOMENA IN MOTOR PERFORMANCE

There is a striking similarity in various reported curves of motor performance. Figures 1, 2, 3, and 4 show performance with Snoddy's mirror-drawing apparatus and the Koerth-type rotary pursuit apparatus. Performance after rest is characterized in each case by (1) an abrupt rise to a level much higher than that to be expected if there had been no rest, (2) a gradual flattening off leading to a relatively decremental segment after this abrupt rise, (3) resumption of the pre-rest curve of gradual improvement, or at least some signs of this, and (4) a fairly definite 'permanent' difference in performance levels between groups practicing under different conditions of distribution of practice. Figures 2 and 3 show the re-appearance of the pre-rest performance curve most clearly. The apparently permanent difference in 'final' levels appears in Figs. 1 and 4.

All these characteristics are not present in all learning curves. They seem to appear most clearly in learning of motor tasks where blocks of massed trials are separated by rests of several minutes, and where the task is a novel one in the fairly early stages of learning. The significance of these conditions will become more apparent

[1] This article is based on a dissertation presented in partial fulfillment of the requirements for the Ph.D. degree, State University of Iowa, 1946. The author wishes to express his gratitude to Professor C. E. Buxton for encouragement and direction of this research, to Professor Gustav Bergmann for mathematical assistance, and to Professors Kenneth W. Spence and G. G. Thompson for critical reading of the manuscript.

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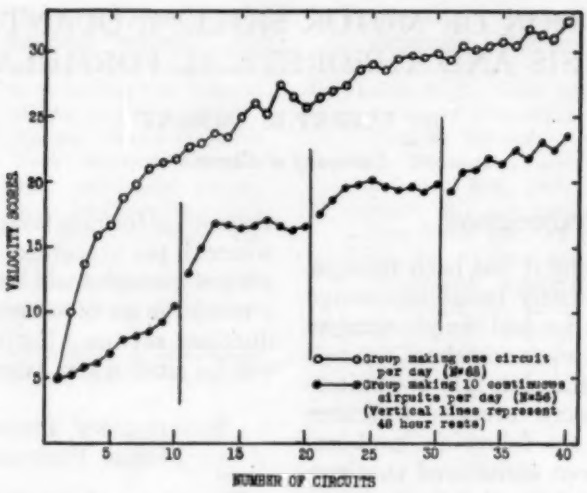
VELOCITY SCORES
NUMBER OF CIRCUITS
Group making one circuit per day (N=65)
Group making 10 continuous circuits per day (N=56)
(Vertical lines represent 48 hour rests)

FIG. 1. Performance in mirror tracing by college students working under different conditions of distribution of practice (redrawn from Snoddy, 17).

in the following discussion of 'theories.' It might be noted that in these curves we seem to be dealing with performance phenomena noticed at least as long ago as the early 1900's, and called learning, initial spurt, warm-up, and fatigue.

EXISTING THEORIES APPLIED TO SELECTED POST-REST MOTOR PERFORMANCE PHENOMENA

One test of theoretical formulations is the degree to which, and the ease with which, they account for available data. McGeoch (11) has reviewed a number of what might be called single-factor explanations of reminiscence, such as rehearsal and recovery from fatigue. However, any curve with two inflections or more[2] cannot very adequately be accounted for by the simple operation of one factor. Several variables probably are involved, although of course any one may be more important than another in particular phases of the performance period. In this section certain 'theories' or suggested sets of variables will be discussed as applied to the four characteristics of motor performance curves already noted. An examination will be made of the tentative sets of variables suggested by Snoddy (15, 17), Doré and Hilgard (4, 5), Bell (1), Melton (12), and Hull (7, 8, 9) as being the most promising for our purposes. It should be made clear at the outset that these formulations were not originally intended to be exhaustive, or even applicable to the present problem. Each will be outlined briefly, and then applied essentially as originally stated. Examination of the 'new' set of variables proposed in this paper will show that many conceptual properties from these previous formulations are utilized.

Primary and secondary growth. Snoddy (15, 17) proposes to account for various learning phenomena in terms of primary and secondary growth. Primary growth appears early in learning and is relatively stable. Secondary growth appears later and is highly unstable. *Early*

[2] If we disregard the rest interval, a third inflection in the performance curve is found at the beginning of practice after rest.

and *late* are relative to the sequence being studied: a day's practice, a month's practice, or a single sequence of trials within a day's practice. Stability is defined in terms of smallness of performance loss over rest periods, and the tendency for the cumulative mean never to fall lower than its level at a previous trial. Primary growth is tentatively defined as the level represented by the cumulative mean for successive trials. Primary growth is a positive function of repetition, and of amount of time interpolated between trials or practice sessions. Secondary growth is the difference on any trial between the actual performance level and the cumulative mean at that point. Secondary growth is a positive function of the amount of primary growth base and lateness in practice, and a negative function of length of

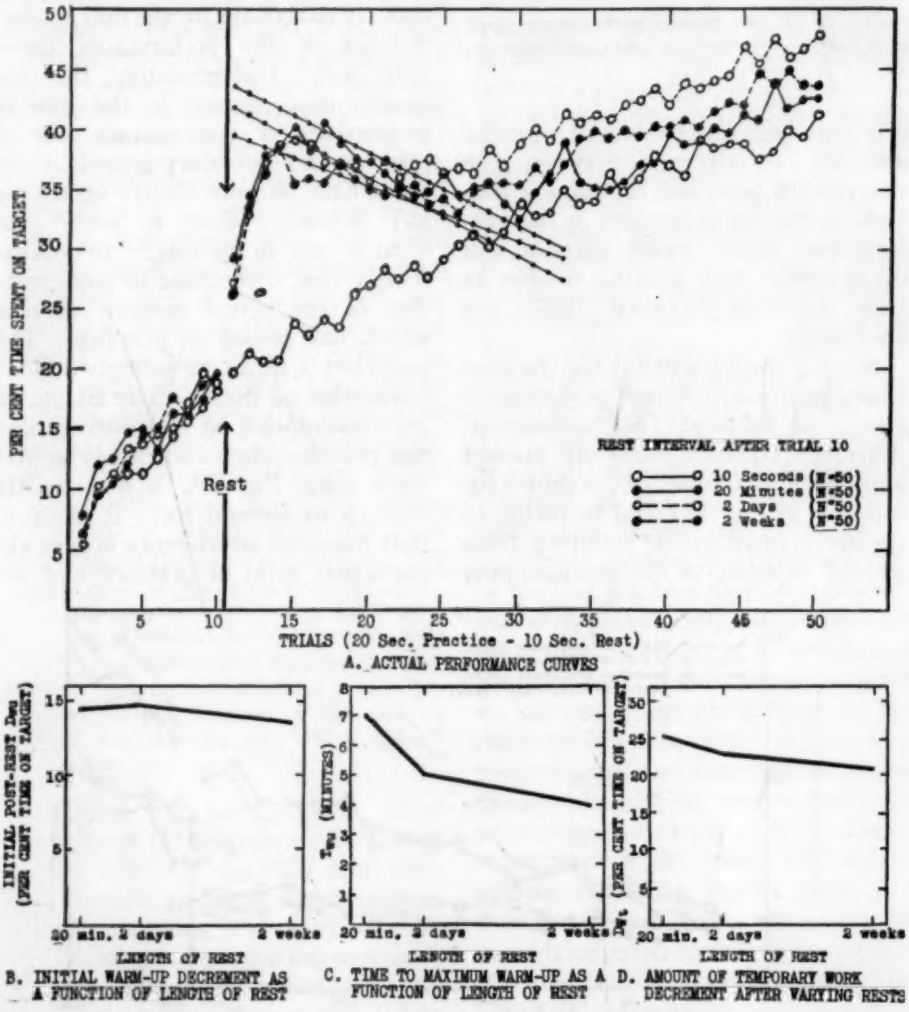
PER CENT TIME SPENT ON TARGET
Rest
REST INTERVAL AFTER TRIAL 10
10 Seconds (N=50)
20 Minutes (N=50)
2 Days (N=50)
2 Weeks (N=50)
TRIALS (20 Sec. Practice - 10 Sec. Rest)
A. ACTUAL PERFORMANCE CURVES
INITIAL POST-REST Dw (PER CENT TIME ON TARGET)
20 min. 2 days 2 weeks
LENGTH OF REST
B. INITIAL WARM-UP DECREMENT AS A FUNCTION OF LENGTH OF REST
Tw (MINUTES)
C. TIME TO MAXIMUM WARM-UP AS A FUNCTION OF LENGTH OF REST
Dw (PER CENT TIME ON TARGET)
D. AMOUNT OF TEMPORARY WORK DECREMENT AFTER VARYING RESTS

FIG. 2. Rotary pursuit performance of men college students when a rest interval is introduced (*A* re-drawn from Melton, 12; *B*, *C*, and *D* derived from *A* by present author using the procedures outlined in this paper).

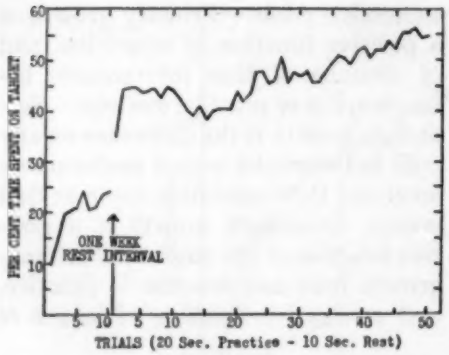

FIG. 3. Rotary pursuit performance of 25 men college students (re-drawn from Pomeroy, **13**).

intervals between trials and practice sessions. Interference between the two growth processes produces a plateau or relative decrement in the performance curve, which plateau will occur earlier in a practice session as time intervals between trials are shortened.

Snoddy might account for the four characteristics of motor performance curves as follows: The 'permanent' difference between relatively massed and distributed practice (as shown for example in Fig. 1) could be owing to excessive interference resulting from greater massing, or the handicapping of primary growth by massing. The sharp initial rise in the post-rest performance curve could be due to initial rapidity of secondary growth after considerable loss during rest. The relatively decremental section would be the result of interference between primary and secondary growth. The subsequent rise would be due to the lessening of interference later on in the same practice day.

Snoddy's formulation seems qualitatively to account for the four characteristics of the performance curve quite well. Unfortunately, the constructs used, except in the case of proportions of performance due to primary and secondary growth at any given time, are not clearly operationally defined. What is 'early' and what is 'late' in learning? In order to specify this, there must be some mention of the actual amount of time which has passed in practice. This turns out to be an exceedingly difficult connection to make. The characteristic resumption of rise during post-rest practice, shown so clearly in Melton's data, Fig. 2A, is also a little difficult to account for. It might be that maximal interference occurs at a particular ratio of primary and sec-

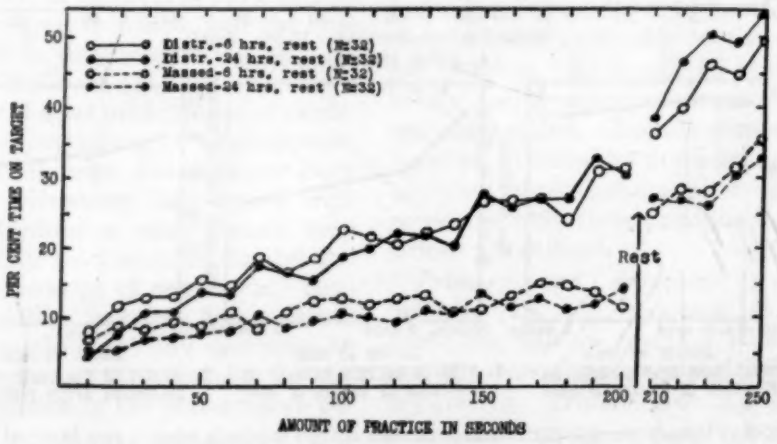

FIG. 4. Rotary pursuit performance of women college students with continuous and distributed practice (10-sec. work, 20-sec. rests; re-drawn from Reyna, **14**).

ondary growth and decreases after that point in practice is reached. The nature of the definitions of the variables makes quantification difficult. Without absolute quantification, detailed prediction will not be possible.

Stimulation-maturation. Doré and Hilgard formulate the stimulus-maturation hypothesis as follows:

> "Stimulation-maturation assumes that there is growth between practice periods, but that this growth is related to the stimulation provided within the practice periods. Scores are a function of time, however, rather than a function of amount of practice (granted the necessary minimum)" (4, p. 258).

This formulation as it stands is unable to account for the four post-rest motor performance phenomena. Its basic assumption is that within wide limits of distribution of practice, all performance curves plotted against total elapsed time from the start of practice will reach about the same level. Reyna's curves (Fig. 4) show that marked differences in performance curves over 6- and 24-hour periods appear with even relatively minor variations in distribution. As Doré and Hilgard themselves point out, the stimulation-maturation hypothesis

> ". . . can be shown to be necessary only if well known processes describing increments and decrements between trials are found to be quantitatively insufficient to account for the changes which occur" (4, p. 259).

Learning and work factors. Doré and Hilgard (5) propose an analysis of Snoddy's mirror drawing and their own pursuit performance curves in terms of learning and work factors. The learning factors are improvement within practice (acquisition) and loss with non-practice (forgetting). The work factors are loss within practice (work-decrement) and improvement with non-practice (recovery with rest). This set of variables accounts for rapid initial post-rest rise of performance in terms of the rapid relearning of habits lost during the rest period. Decrement within practice is accounted for in terms of work-decrement. Rise after relative decrement in the same practice session is not accounted for, nor is the relatively permanent difference in performance between groups with different degrees of distribution of practice. As Doré and Hilgard themselves point out, this group of factors is principally classificatory in nature, and insufficient as a basis for prediction. It is hard to say whether or not their hypothesis could account for the data if elaborated.

Melton (12) also proposes that rotary pursuit performance be analyzed in terms of the interaction of learning and forgetting, and of work decrement and recovery from work. He identifies a marked improvement in his curves during the first few trials of post-rest practice, and suggests that this improvement is due to some sort of forgetting process during rest superimposed on the beneficial effects of rest. The subsequent decremental portion of the post-rest curve is noted; in the last segment there is a resumption of approximately the pre-rest rate of improvement and projected level of performance. As no attempt is made to interpret these phenomena, or the different levels reached under relatively massed and distributed practice the adequacy of the system cannot at present be evaluated. Melton recognizes three of the major phenomena pointed out earlier in this paper, but does not believe that a theory can be formulated until more generalizations of a quantitative nature are available.

Bell (1), after analyzing the results of his experimental test of Snoddy's hypothesis, proposes a tentative theory to account for changes during rotary

pursuit practice. He assumes a general course of improvement, on which are superimposed an interference and warming-up factor.

"On the pursuit rotor, interference and warming-up may be inferred to operate throughout the course of learning with different effects at different stages. Interference is assumed to be greatest in the first few trials and to become less as learning progresses, while warming-up is inferred to increase rapidly during the first few trials but then to remain fairly constant throughout the rest of the learning. Rest intervals tend to remove interference but necessitate warming-up for subsequent trials" (1, p. 35).

Bell's formulation accounts for the initial sharp post-rest rise in terms of 'warming-up.' It accounts for the decremental segment only indirectly, in terms of a factor called interference. It does not account for post-rest resumption of gain and persistent differences in level of performance due to degree of distribution of practice. Operational definitions of warming-up and interference are given, in terms respectively of gain between the first and second post-rest trials and sizes of the standard deviation of individual scores on successive trials.

The last formulation to be considered is a general behavioral one proposed by Hull (8). Hull outlines a system including inhibition of reinforcement (a temporary work decrement) and conditioned inhibition (a permanent decremental factor). Inhibition of reinforcement or reactive inhibition (I_R) is a positive function of number of reactions and the amount of actual work involved. I_R diminishes progressively with the passage of time after cessation of work. Conditioned inhibition ($_SI_R$) is a learned reaction of not-responding developed on the basis of the 'negative drive state' of I_R (*i.e.*, a desire to stop working). Reminiscence will occur over rest periods as the temporary decremental effect of I_R disappears.

Hull's general constructs at their present stage of development account for only certain of the four motor performance curve characteristics. Obviously, the greater the massing of practice, the greater the I_R, therefore the greater the $_SI_R$ or permanent decrement. This will then account for the seemingly persistent difference in performance level between distributed and massed practice groups, even after reminiscence has taken place (Fig. 4). Gain over rest is accounted for in terms of dissipation of I_R. The specific post-rest phenomena of sharp initial rise, relative decrement, and resumption of rise are not directly accounted for.

Summary of discussion of theories. No one of the theoretical formulations examined thus far appears adequate as originally stated to account for all particular phenomena noted. On the other hand, several potentially useful variables have been mentioned: temporary work decrement, permanent work decrement, and warming-up. The next step is to choose a promising set of variables and to define them in terms of a motor performance curve. In the following discussion it will be seen that the author is indebted to many others for the basic ideas behind the variables, particularly Hull, Bell, Hilgard, and Melton.

ISOLATION AND DEFINITION OF VARIABLES

The schematic drawing in Fig. 5 shows the variables on which the present theoretical system is based.

A—represents the curve of performance if no special rest period is introduced. This could be obtained either by the use of a control group or by

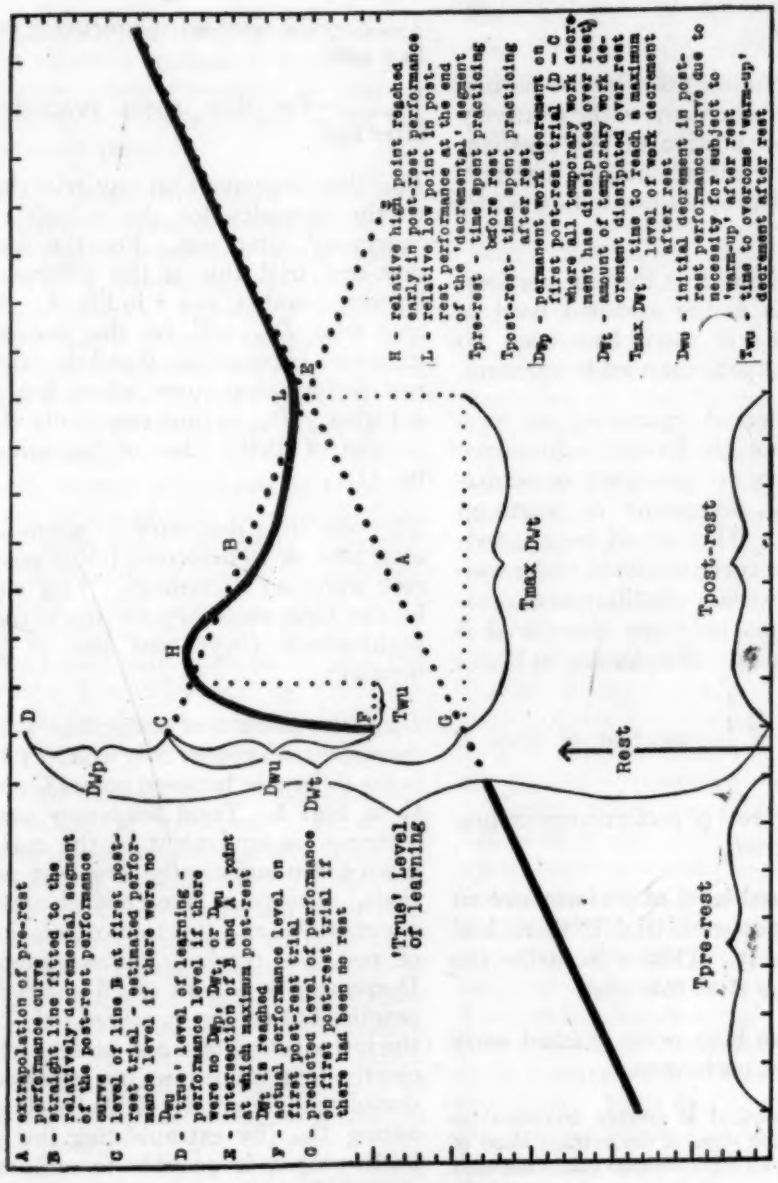
A - extrapolation of pre-rest performance curve
B - straight line fitted to the relatively decremental segment of the post-rest performance curve
C - level of line B at first post-rest trial - estimated performance level if there were no Dwu
D - 'true' level of learning - performance level if there were no Dwp, Dwt, or Dwu
E - intersection of B and A - point at which maximum post-rest Dwt is reached
F - actual performance level on first post-rest trial
G - predicted level of performance on first post-rest trial if there had been no rest
H - relative high point reached early in post-rest performance
L - relative low point in post-rest performance at the end of the 'decremental' segment
Tpre-rest - time spent practicing before rest
Tpost-rest- time spent practicing after rest
Dwp - permanent work decrement on first post-rest trial (D - C where all temporary work decrement has dissipated over rest)
Dwt - amount of temporary work decrement dissipated over rest
Tmax Dwt - time to reach a maximum level of work decrement after rest
Dwu - initial decrement in post-rest performance curve due to necessity for subject to 'warm-up' after rest
Twu - time to overcome 'warm-up' decrement after rest
'True' Level of learning
Dwp
Dwu
Dwt
Twu
Tmax Dwt
Rest
Tpre-rest
Tpost-rest
LEVEL OF PERFORMANCE
TIME

FIG. 5. Representations of certain rotary pursuit performance variables.

extrapolation of the pre-rest performance curve. Extrapolation is used and will be done by visual estimate and the resulting curves indicated by 'dashed' lines.

B—a straight line fitted by the method of average decrement[3] to the relatively decremental portion of the post-rest performance curve, namely, that portion bounded by and including points *H* and *L*.

C—level of line *B* at the first post-rest trial. This is the assumed level of performance if there had been no decrement due to the need to warm-up.

D—hypothetical maximum of performance, *i.e.*, the level of performance if there were no temporary or permanent work decrement or warm-up decrement. This would be approximated by a curve obtained under conditions of extreme distribution of practice as when only one short trial is given per day. *D* is similar to Hull's ${}_{S}E_{R}$ (8).

E—point of intersection of lines *B* and *A*.

F—actual level of performance on first post-rest trial.

G—predicted level of performance on the first post-rest trial if there had been no rest. This is actually the level of line *A* at this trial.

H—relative high point reached early in post-rest performance.

[3] In the method of average decrement as here used, the slope is the average slope of lines joining all adjacent trial values included within the segment of the curve being fitted. This segment is defined as that bounded by and including the highest and lowest single points on the curve. With slope determined, the line is assumed to pass through the point of intersection of the lines representing the vertical and horizontal means of these values.

L—'low' point in post-rest performance, at the end of the relatively decremental segment.

$T_{pre\text{-}rest}$—the time spent practicing before rest.

$T_{post\text{-}rest}$—the time spent practicing after rest.

D_{wu}—the decrement on any trial due to the necessity for the subject to 'warm-up' after rest. For the first post-rest trial this is the difference between points *C* and *F* in Fig. 5. At any trial D_{wu} will be the vertical difference between line *B* and the post-rest performance curve where line *B* is higher. D_{wu} is thus essentially the inverse of Bell's idea of 'warming-up' (1).

T_{wu}—the time necessary to warm-up after rest, or to overcome initial post-rest warm-up decrement. This will be the time necessary for the actual performance curve and line *B* to intersect.

D_{Wt}—the amount of temporary work decrement *dissipated* over rest. This is the difference between points C and G in Fig. 5. Total *temporary work decrement* is equivalent to the maximum given sufficiently long rest periods. This point is reached when the curve of D_{Wt} as a function of duration of rest has reached its asymptote. Decrement is present at all points in practice but can be measured only by the introduction of a rest period sufficiently long to insure its relatively complete dissipation. Then by eliminating D_{wu} (by extrapolating line *B* backward) it is possible to estimate the total temporary work decrement present at the end of the last pre-rest trial. *Reminiscence* or gain over rest is due to this dissipation of temporary work decrement. No implication is intended that temporary work decre-

ment is due to fatigue as commonly defined. D_{Wt} can be seen to be similar to Hull's I_R.

$T_{max}\ D_{Wt}$—the time necessary to reach an effective maximum level of temporary work decrement in post-rest practice. This is defined as the time to reach point *E*.

D_{Wp}—permanent work decrement. On the first post-rest trial this will be the difference between points *C* and *D* in Fig. 5, providing there is no temporary work decrement remaining after rest. If some temporary work decrement remains after rest, part of the difference between *C* and *D* will be due to its presence. There will be an amount of D_{Wp} at every point in performance, which can be measured only by introducing a rest to eliminate the decremental effects of temporary work decrement completely and comparing initial post-rest performance corrected for D_{wu} with that of a control group with short trials and long rests. The difference, by definition, would be due to D_{Wp}. D_{Wp} is thus similar to Hull's ${}_sI_R$ (**8**).

This completes the isolation and definition of variables. The actual level of performance in this system is arrived at subtractively; *i.e.*, permanent work decrement and total temporary work decrement are subtracted from the hypothetical maximum level of performance. This subtractive procedure has led to the use of 'decrement due to the need to warm-up' rather than the possibly simpler concept of 'warming up' in order to maintain consistency.

RELATIONSHIPS BETWEEN VARIABLES

The purpose of the present section is to interrelate certain major variables in rotary pursuit learning within a series of assumptions, as a first step toward a comprehensive theory of motor learning. Wherever possible, assumptions will be based on actual data, but since so few data are available, certain assumptions will merely be tentative proposals based on very indirect evidence. Most of the graphs from other studies, from which data have been derived, are presented, and the basic lines from which derived measures have been obtained have been drawn in. Performance at any point is a function of at least the following independent variables: number of previous practice sessions, durations of previous practice sessions, durations of rests between sessions, distribution of practice within practice sessions, and duration of practice so far in the current practice session. No attempt has been made to construct an exhaustive or comprehensive set of assumptions or to present more than a few of the possible deductions. The discussion following each of the assumptions and deductions is not intended to add anything to the logical properties of the system, but rather to give collateral evidence or illustrate the process of reasoning used.

Certain terms should be explained before they are used: work-rest trial cycle, practice session, inter-trial rest, intersession rest. These may perhaps best be clarified by application to a hypothetical situation. A group of subjects practices 20 seconds and rests 10 seconds. This is a *trial* cycle. The *inter-trial rest* is 10 seconds. This trial cycle is repeated 10 times on each of four days. Each day's total practice is a *practice session*. The *intersession rest* is approximately 24 hours.

Assumption 1. *Initial post-rest* D_{wu} *increases as a negatively accelerated increasing function of total duration of previous practice.*

Suppose that warm-up decrement is owing to loss during rest of 'set,' con-

sisting principally of various advantageous postural adjustments. The more practice before rest the more of these will have been acquired, and the more there will be to lose. The course of acquisition of advantageous postural adjustments should follow a typical negatively accelerated learning curve.

Evidence for this generalization comes from Bell (1), who introduced rests of varying lengths after 5 and 15 minute rotary pursuit practice. Bell measured warm-up in terms of the gain between the first two post-rest trials, and found the amount of gain or warm-up after the lesser amount of pre-rest practice to be slightly greater. When one subtracts the amount of increment to be expected at these levels of practice if no rest had been introduced (using Bell's basic 'learning curve' equation; 1, pp. 9–10), there is more warm-up after the greater amount of pre-rest practice (Figs. 6A and B). Bell's 'warming-up' is our 'overcoming of D_{wu}.'

Assumption 2. *D_{wu} will be a positively accelerated decreasing function of the duration of practice since intersession rest.*

It would seem that warming-up or 'set' recovery would be more rapid at first after rest, then taper off. In actual performance, this would mean that the successive ordinate differences between line *B* and the actual performance curve in Fig. 5 will constitute a negatively accelerated decreasing curve. Inspection of the actual performance curves in this paper seems to lend some support to this generalization, and Fig. 2A shows the phenomenon clearly.

Assumption 3. *The 'shape' of the curve of D_{wu} within a practice session will always be the 'same,' whatever the initial amount of D_{wu}.*

This is the simplest possible assumption. It is assumed that the recovery process is essentially the same wherever it occurs. 'Same shape' refers to the following mathematical properties: The first quadrant of the *xy* plane is 'simply' covered by the family of curves. Consideration is restricted to those segments of the curves where the amount of D_{wu} is appreciable, *i.e.*, D_{wu} is observable. The

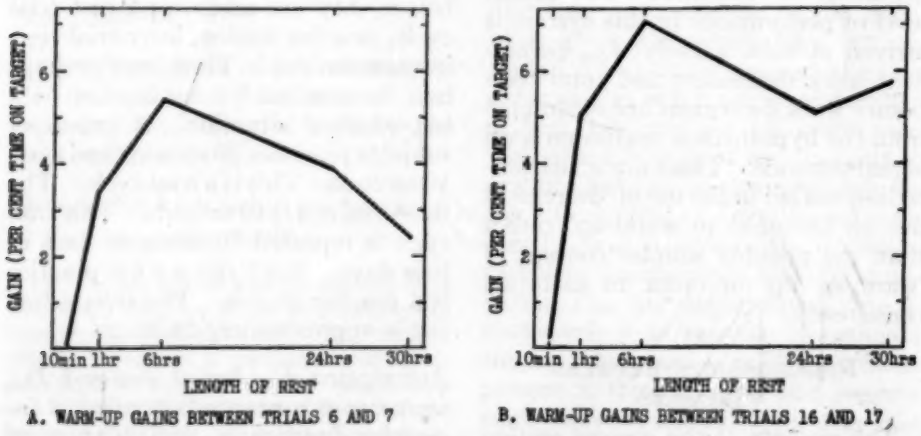

FIG. 6. Post-rest gains due to warming-up after the introduction of rests of varying lengths after 5 or 15 1-min. practice, 1-min. rest cycles of rotary pursuit practice. Groups consist of 40 or more college students. Taken from Bell's Tables 5 and 6 (1) with expected 'learning' increment estimated and subtracted by present author.

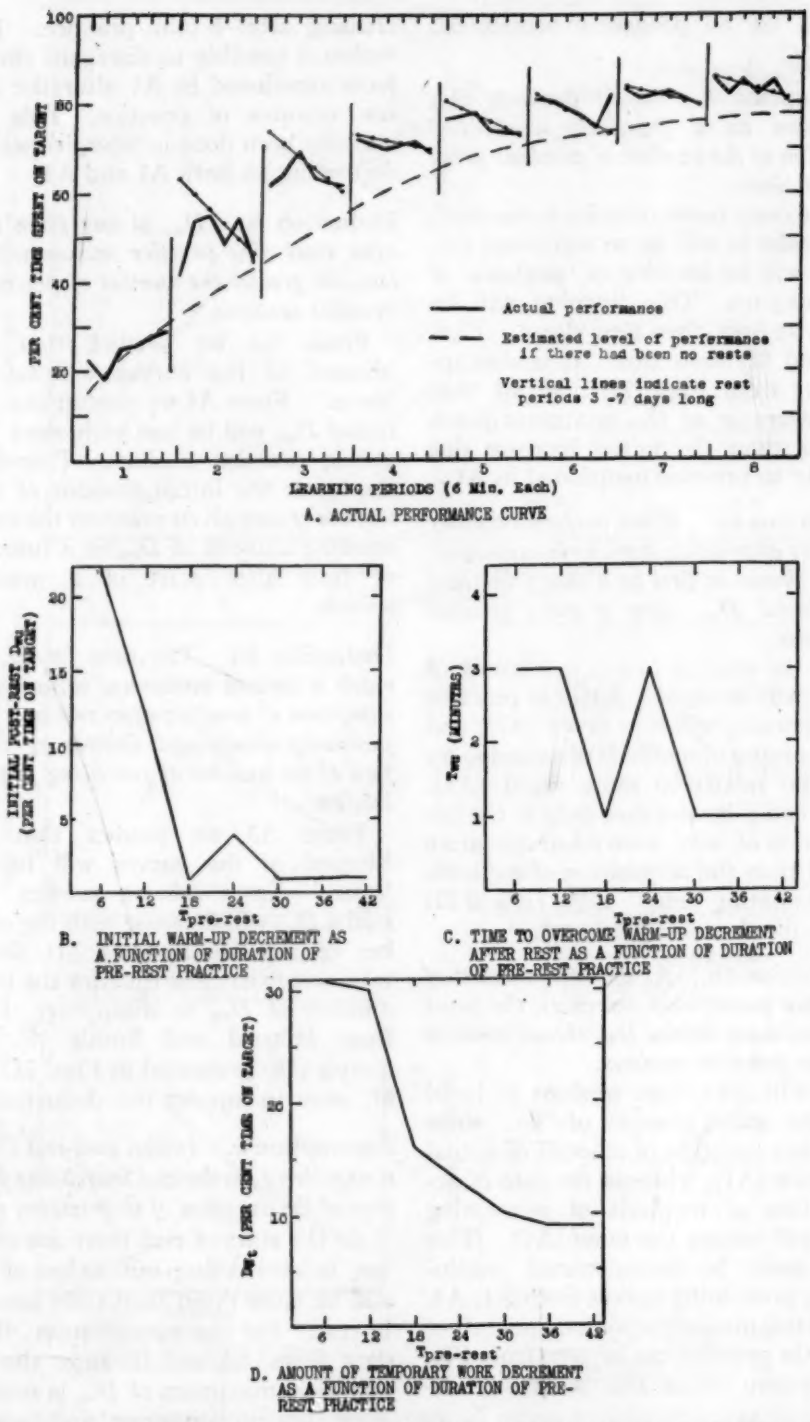

FIG. 7. Rotary pursuit performance of four male college students (*A* re-drawn from Travis, **18**; *B*, *C*, and *D* derived by present author from *A* using the procedures outlined in this paper).

curves are all positively accelerated (A4).[4]

Assumption 4. *Initial post-rest D_{wu} decreases as a positively accelerated function of the number of previous practice sessions.*

The more times practice is resumed, the easier it will be to warm-up; *i.e.*, there will be learning of 'methods' of warming-up. This learning will be rapid at first, then slow down. Figs. 7B and 8B show this. It is also apparent from Figs. 7B and 8B that this decrease of D_{wu} continues much longer than the initial increase due simply to practice mentioned in A1.

Deduction 4a. *When performance over a series of practice sessions is examined, there should at first be a sharp increase in initial D_{wu}, then a more gradual decrease.*

At the start of practice, learning of 'sets' will be rapid. Later in practice this learning will slow down (A1), and the learning of methods of warming up will be relatively more rapid (A4). This could be the case only if the acquisition of 'sets' were relatively more rapid than the acquisition of methods of reinstating 'sets.' Figs. 7B and 8B show the decrease predicted.

Deduction 4b. *As the total amount of practice per session decreases, the point of maximum initial D_{wu} should come at earlier practice sessions.*

It will take more sessions to build up the same amount of 'set,' since 'set' is a function of amount of actual practice (A1), whereas the rate of acquisition of methods of reinstating sets will remain the same (A4). This can easily be demonstrated graphically, combining curves from A1, A4. That this maximum point comes rather early in practice can be seen from Fig. 7B, where initial D_{wu} is already decreasing after 6 min. practice. This makes it possible to disregard the effects mentioned in A1 after the first few minutes of practice. This has actually been done in later deductions depending on both A1 and A4.

Deduction 4c. *D_{wu} at any given time after start of a practice session will be less, the greater the number of preceding practice sessions.*

From A3 we predict that the 'shapes' of the curves will be the 'same.' From A4 we predict that the initial D_{wu} will be less with more preceding practice sessions. Therefore, the lower the initial amount of D_{wu}, the lower any given point on the curve showing amount of D_{wu} as a function of time after start of a practice session.

Deduction 4d. *The time for D_{wu} to reach a certain minimum value on resumption of practice after rest will be a positively accelerated decreasing function of the number of preceding practice sessions.*

From A3 we predict that the 'shapes' of the curves will be the 'same.' From A4 we predict that initial D_{wu} will decrease with the number of practice sessions. It should take somewhat less time for the lesser amount of D_{wu} to disappear. Data from Hilgard and Smith (6) and Travis (18) presented in Figs. 7C and 8C seem to support this deduction.

Assumption 5. *Initial post-rest D_{wu} is a negatively accelerated increasing function of the duration of intersession rests.*

At the start of rest there are many 'set' factors to drop out, so loss of 'set' will be more rapid then than later on in rest. For the pursuit rotor, Bell's data (Figs. 6A and B) show that an empirical maximum of D_{wu} is reached after about 6 hours rest, and most of this appears after the first hour.

[4] From this point on Assumption will be abbreviated as A, and Deduction as D.

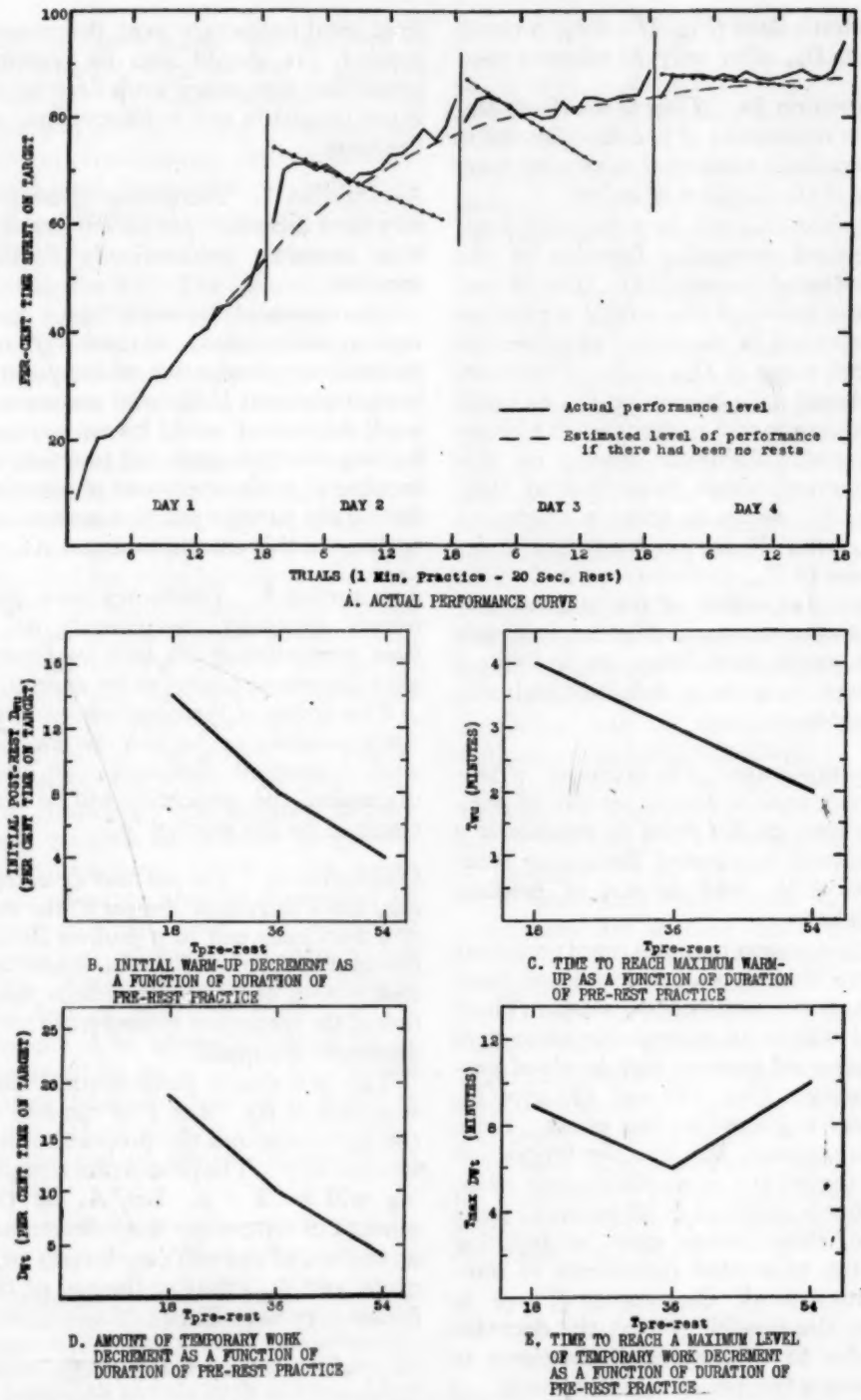

FIG. 8. Rotary pursuit performance for groups of college students practicing 18 1-min. trials on each of four days. The second and last trials on each day followed 1-min. rests, while the remainder followed 20-sec. rests. (*A* re-drawn from Hilgard & Smith, 6; *B*, *C*, *D*, and *E* derived by present author from *A*).

Melton's data (Fig. 2B) show a maximum D_{wu} after only 20 minutes rest.

Deduction 5a. *Time to eliminate D_{wu} upon resumption of practice after rest is a negatively accelerated increasing function of the duration of the rest.*

Initial D_{wu} will be a negatively accelerated increasing function of the duration of the rest (A5). The 'shape' of the curve of D_{wu} within a practice session will be the 'same' whatever the initial value of D_{wu} (A3). Therefore, it should take longer for D_{wu} to reach a minimum value after rest the longer the rest. Evidence bearing on this deduction comes from Melton (12). Fig. 2C seems to show a maximum T_{wu} after 20 minutes rest and a decrease in T_{wu} thereafter up to 2 weeks rest. Inspection of the original performance curves in Fig. 2A indicates too much variability in individual curves to make a definite conclusion possible.

Assumption 6. *The increment of temporary work decrement per unit of practice time at any point in practice is a positively accelerated decreasing function of the total amount of previous practice.*

As a person becomes more proficient there should be less decrement from the same amount of work. There will still be an appreciable amount of decrement even at high levels of proficiency. Figs. 7D and 8D give indirect evidence on this point. Since it is assumed (A8) that the proportion of temporary work decrement dissipated is constant at all points in practice, these curves may be declining owing to smaller increments of temporary work decrement. There is also the possibility that the decrease is due to a flattening of the curve as it nears 100 per cent performance.

Increment of temporary work decrement should be carefully distinguished from total temporary work decrement present. It should also be remembered that temporary work decrement is not fatigue in any ordinary sense of the term.

Assumption 7. *Increments of temporary work decrement per unit of practice time cumulate arithmetically during practice.*

This represents no more than a convenient assumption. If there were no concomitant dissipation of temporary work decrement (A8), total temporary work decrement would be an increasing negatively accelerated function of number of trials or amount of practice during the current practice session according to this assumption and A6.

Assumption 8. *Temporary work decrement dissipates continuously as a fixed proportion of the total temporary work decrement present at the instant.*

This is one of the simplest assumptions possible as to how temporary work decrement dissipates. In later discussion, the proportion will be represented by the symbol 'p.'

Deduction 8a. *The net level of temporary work decrement present at the end of a trial cycle will be a positive linear function of the size of the increment per unit of time, and an inverse linear function of the proportion of temporary work decrement dissipated.*

This is a simple mathematical consequence of A8. Let I be the size of the increment and the proportion dissipated be p. The proportion remaining will be $1 - p$. Let A_n be the amount of temporary work decrement at the end of the nth (work-rest) trial cycle, and A_{n-1} that at the end of the previous cycle. Then

$$A_n = (A_{n-1} + I)(1 - p),$$

which is a linear equation when p remains constant. This same equation

can be used where the proportion dissipated (p) varies and I remains constant. It should be apparent that this and the following equations are only approximations, since both increment and dissipation of temporary work decrement take place during practice.

Deduction 8b. *The net level of temporary work decrement developed serially during a practice session will be a negatively accelerated increasing function of the amount of practice since the start of that session.*[5]

The amount dissipated per unit of practice is a constant proportion of the amount present (A8). Since the amount present will be increasing, provided less than 100 per cent is dissipated, the absolute amount dissipated per unit of practice will increase until an equilibrium is reached between increment and decrement—where the amount dissipated per practice unit is equal to the increment. It can be seen from this that the net increment per trial-cycle will become increasingly smaller as practice progresses, since increment remains the same, while absolute amount dissipated increases. This could be expressed roughly in mathematical form as follows. Using the notation of D8a, at the end of the first trial-cycle the amount A of temporary work decrement will be $A_1 = I(1 - p)$. At the end of the second trial-cycle, it will be $A_2 = I(1 - p)(1 - p) + I(1 - p)$. At the end of the nth trial-cycle, it will be

$$A_n = I(1 - p)^n + \cdots + I(1 - p)^2 + I(1 - p)$$

or

$$A_n = I[(1 - p)^n + \cdots + (1 - p)^2 + (1 - p)].$$

From this equation, it should be apparent that with increasing trials the net increment of temporary work decrement will become smaller—on the nth trial the net increment will be $I(1 - p)^n$, and on the next trial it will be $I(1 - p)^{n+1}$, a smaller amount, since $1 - p$ is always less than one by definition. Empirical evidence could be obtained by introducing rests of a constant length after varying numbers of trials to see if the total amount of temporary work decrement reaches a maximum and flattens out.

Deduction 8c. *The greater the proportion of temporary work decrement dissipated per work-rest cycle during a practice session, the lower the effective maximum level of temporary work decrement which will be reached.*

From Deduction 8a (D8a) we predict that net increments per trial-cycle will be less, the greater the proportion of temporary work decrement dissipated per trial-cycle. Since the net increments are smaller and remain proportionately smaller with a greater proportion of dissipation, the maximum total level reached will be lower. Consideration will show that the larger the proportion of dissipation the sooner the stage is reached where this proportion of total temporary work decrement is approximately equal to the increment. Take the equation for total temporary work decrement present, from D8b, where

$$A_n = I[(1 - p)^n + \cdots + (1 - p)^2 + (1 - p)].$$

Simple arithmetic will show that on the nth trial, the larger p has been, the smaller A_n will be.

[5] Although it may seem so, this is not contradictory to Hull's assumption of linear increase in I_R. One actually can obtain the present deduction within Hull's system by combining his formula for I_R as a function of amount of work and number of trials with his principle of dissipation of I_R as a simple decay function (8, pp. 278–281).

Deduction 8d. *The proportionately longer the rest per work-rest trial cycle during a practice session, the lower the effective maximum level of temporary work decrement which will be reached.*

The proportionately longer the rest per work-rest trial cycle, the greater the proportion of temporary work decrement which will dissipate since more time will be available for dissipation (D8a). From D8c we can then predict that the maximum level of temporary work decrement will be lower with proportionately longer rests per trial cycle.

Deduction 8e. *The proportionately longer the rest per work-rest trial cycle during a practice session, the less practice will be needed to reach an effective maximum level of temporary work decrement.*

Since the net increments per trial cycle will be smaller (D8a) with longer rests, after any given amount of practice, it will take less time to reach an effective maximum defined as a net increment smaller than a certain absolute amount. Take the equation for total temporary work decrement after n trials:

$$A_n = I[(1-p)^n + \cdots + (1-p)^2 + (1-p)].$$

Simple arithmetic will show that it will take many fewer trials for the net trial increment on trial n, $I(1-p)^n$, to reach a value of say .01 I where $p = .9$ than where $p = .5$. In the case of $p = .9$, this point will be reached after two trials, while it will be reached only after seven trials were $p = .5$.

Deduction 8f. *The curve showing dissipation of temporary work decrement over rest (D_{Wt} as a function of duration of rest) will be a simple negatively accelerated increasing function of the duration of the rest period.*

During rest no temporary work decrement is being added. A constant proportion of the amount already present at each successive instant will dissipate. This will produce a negatively accelerated curve of recovery (D_{Wt}), since there will be increasingly smaller amounts added. If A_i is the amount of temporary work decrement present at the start of rest, and A_{D_n} the amount dissipated after n units of time, then:

$$A_{D_n} = A_i(p + p^2 + \cdots + p^n).$$

The additional amounts dissipated will be increasingly larger powers of p, and so will have increasingly smaller absolute values.

A hint as to the general nature of this curve comes from Melton's data (**12**), Fig. 2D. Here temporary work decrement apparently has dissipated completely over a rest of 20 minutes. Indirect evidence also comes from Troyer's (**20**) and Travis' (**19**) studies. As will be argued under D11a, the levels of performance reached will be an inverse function of the amount of temporary work decrement present after rests interpolated between continuous practice sessions. Troyer found that inter-trial rests of 5 and 9 minutes gave higher levels of performance than shorter rests, but 9 minutes gave little better than 5 minutes. This would seem to indicate that temporary work decrement built up during practice on the Renshaw-type pursuit meter dissipates almost completely during about 10 minute rests. Travis found (Figs. 9A, 9B) only slightly better performance with inter-trial rests of 20 minutes than with 5 minutes. This seems to indicate rather complete dissipation of temporary work decrement during about 5 minutes rest after pursuit oscillator practice.

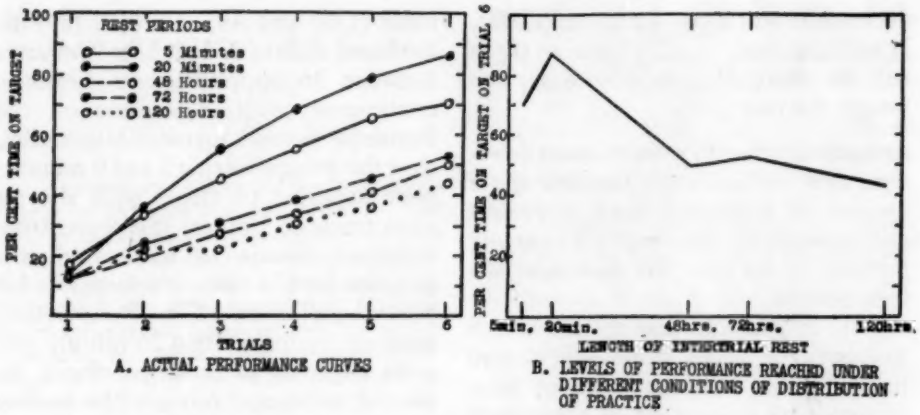

FIG. 9. Pursuit oscillator performance of college men with 5-min. practice periods and different lengths of inter-trial rests (re-drawn from Travis, 19).

Deduction 8g. *The longer the rest between practice sessions, the lower the initial level of temporary work decrement at the start of each new session.*

This follows directly from D8f. The longer the rest, the more temporary work decrement dissipated, so the less remaining at the start of a new session.

Deduction 8h. *The shorter the rest between practice sessions, the less the amount of practice it will take in a new session to build to a maximum of temporary work decrement.*

The maximum possible temporary work decrement under given conditions is fixed by factors independent of the length of rest between sessions, particularly the work-rest ratio per trial (D8d). The shorter the rest, the more temporary work decrement present at the start of a new practice session. The more the temporary work decrement at the start of practice, the less practice time it will take to build up to a maximum, since this temporary work decrement can be assumed to be equivalent to that which would have been built up by practice (D8b). The effect of this temporary work decrement initially present will be equivalent to that of already having given some practice in the session, and so will decrease the amount of practice for temporary work decrement to build to a maximum.

Using the notation of D8a and representing amount of temporary work decrement present at the start of practice as A_s, the amount on the nth trial will be:

$$A_n = (1 - p)^n(A_s + I) + I(1 - p)^{n-1} + \cdots + I(1 - p).$$

The only difference between this equation and that where no temporary work decrement is present initially ($A_n = I[1 - p)^n + \cdots + (1 - p)^2 + (1 - p)]$ from D8b) lies in the second term $[(1 - p)^n(A_s + I)]$. Since this second term is larger, the larger A_s, this curve will be proportionately higher. It can be seen, however, that the two curves will approach the same limit. From these considerations, it can be shown that the greater the A_s, the sooner in practice the net increment will be reduced to any given size.

Deduction 8j. *The post-rest performance curves after longer and shorter rests will tend to diverge for several trials.*

The curve for the longer rest will be higher, since more temporary work

decrement will have dissipated (D8f). It will rise more rapidly because there will be more D_{wu} to overcome, the longer the rest (A5).

Assumption 9. *Permanent work decrement is a multiplicative function of the amount of temporary work decrement and a negatively accelerated increasing function of the time this decrement has been present.*

This is a simple assumption which can only be given some speculative justification. We can think of temporary work decrement as temporary inadequate practice methods. These will become stronger habits the longer they are practiced. This learning of inadequate practice methods can be assumed to be a negatively accelerated function since this is probably the course of 'true' learning (see Hull's ${}_{S}H_{R}$, 8).

Assumption 10. *'True level of learning' will be a simple growth function of the amount of time spent in practice.*

This is the same sort of construct as Hull's ${}_{S}H_{R}$. In light of his discussion (8), this seems to be the assumption most compatible with present experimental data.

Assumption 11. *Actual performance level at any point in practice will be 'true level of learning' minus permanent work decrement, and temporary work decrement present at that point.*

This derives from the definition of the various factors, and simply expresses concisely their quantitative relationship.

Deduction 11a. *The proportionately shorter the rest per trial cycle, the lower the level of the actual performance curve during the practice session.*

There will be more temporary and permanent work decrement to subtract from hypothetical maximum performance the less the time allowed for dissipation of temporary work decrement (D8d and A9). Troyer (20) introduced rests of 0, 1, 3, 5, or 9 minutes between 36 approximately 4-minute continuous practice sessions on the Renshaw pursuit meter. His finding that the groups resting 5 and 9 minutes were superior to those with shorter rests tends to confirm the present deduction. Travis (19) had his subjects practice for 6 5-minute sessions on his pursuit-oscillator. Fig. 9B shows that sessions separated by a 20-minute rest were superior to those separated by shorter and longer rests. This finding also supports the deduction, although the decline in effectiveness of rests longer than 20 minutes is not accounted for.

SUMMARY

In this paper a start has been made toward a theoretical formulation of motor learning phenomena. A method of analyzing certain basic characteristics of rotary pursuit performance curves has been presented and explained. A series of tentative assumptions has been proposed, to express the possible nature of the more important relationships between the variables.[5]

1. It is assumed that under certain experimental conditions, called 'continuous practice,' a certain intervening variable, called 'inhibition,' (a) increases as a function of the time elapsed since the start of continuous practice, and (b) decreases (dissipates) according to an exponential law e^{-Kt}. Let us call *net* inhibition $[I_n(t)]$ the value of this construct at the time t. By *absolute* inhibition $[I_a(t)]$ is meant the inhibition that would accumulate if no dissipation took place [process (a) only]. The system also proposes an operational definition of $I_n(t)$ in terms of actual learning data. It would then appear that these data allow the numerical determination of the constant K. This gives rise to the following question: Assuming K and $I_a(t)$ to be given, compute $I_n(t)$. The solution is

[5] The author is indebted to Professor Gustav Bergmann for the following mathematical formulation of certain important aspects of the system presented in this paper.

immediate.

$$(1.1)\quad dI_n = I_n'dt - I_nKdt,$$

$$(1.2)\quad I_n' = \frac{dI_n}{dt} + KI_n,$$

$$(1.3)\quad I_n(t) = \int_0^t \left(\frac{dI_n}{dt} + KI_n\right) dt.$$

2. Consider now a situation in which periods of practice alternate with periods of rest. Let $\bar{I}_n(t)$ be the inhibition present t units of time after the start of the first practice period. $\bar{I}_n(t)$ is again the result of two processes. More specifically, it is assumed that: (a) if the first practice period lasts t_1 seconds, the second t_2 seconds, and so on, then the absolute increase of $\bar{I}_n$ during the ith practice period is identical with that of I_n between the two time points $\sum_{\nu=1}^{\nu=i-1} t_\nu$ and $\sum_{\nu=1}^{\nu=i} t_\nu$; (b) dissipation (with the same coefficient K) continues during the rest periods.

This gives rise to the following question: Given a certain distribution of practice and rest, and assuming I_n to be known, compute $\bar{I}_n$. This computation requires the successive solution of a series of ordinary differential equations and will, as a rule, have to be performed stepwise. To give an example, assume that 20 seconds of practice alternate regularly with 40 seconds of rest. In order to sketch the procedure the first three steps are presented:

$$(2.1)\quad \bar{I}_n(20) = I_n(20),$$

$$(2.2)\quad \bar{I}_n(60) = \bar{I}_n(20)e^{-40K},$$

$$(2.3)\quad d\bar{\bar{I}}_n^{(2)} = I_n(t-20)dt - K\bar{\bar{I}}_n^{(2)}dt,$$

$$(2.31)\quad \frac{d\bar{\bar{I}}_n^{(2)}}{dt} + K\bar{\bar{I}}_n^{(2)} = I_n(t-20).$$

The superscript indicates that the relation obtains only during the second practice period $[\bar{I}_n(t) = \bar{\bar{I}}_n^{(2)}(t-60)]$. The boundary condition of (2.31) is given by (2.2). The process has to be iterated for each practice interval.

3. If the practice periods are short and/or if one is ready to neglect dissipation *during* practice periods, one obtains an approximation that can be written down in closed form. Assume, *e.g.*, that 5 seconds practice periods alternate with 55 seconds of rest. At the end of the third minute the computation would be as follows:

$$(3.1)\quad \bar{I}_n(180) = I_n(5)e^{-175K} + [I_n(10) - I_n(5)]e^{-115K} + [I_n(15) - I_n(10)]e^{-55K}.$$

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TRIAL AND ERROR LEARNING

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In modern times, from the sixteenth to the present century, the solution of the problem of knowledge has been sought, almost exclusively, by *a priori* means, or by such means aided at the most by introspective analysis of the higher thought processes of the adult, normal, human individual, uncontrolled by experiment or other laboratory procedures. One has only to recall the names of Descartes, Locke, Berkeley, Hume, Kant, James, and Dewey, to be convinced how much speculation there has been, and how little study of the facts. Moreover, in philosophy, there has been manifest a tendency to consider the knower as something outside the world, and his knowing as that of a transcendental ego, or supernatural being, and not as the act of a conscious organism living within the world. This has led to the feeling on the part of some that no real knowledge could be obtained, either of the knower or of things professed to be known. Such skepticism, lurking near the very heart of modern science and philosophy, has added not a little to the confusion of our times, and let down barriers to the totalitarian philosophy of government and of war, for, if nothing could be known of the realm of nature and its laws, and, more particularly, of those laws of nature which hold within the realm of man's relations to his fellows, then every project which force or guile, or any other means, might be able to accomplish was open to exploitation. For since one man's notion of the world was as true or as false, as good or as bad, as that of any other man, it followed that one man's aims, or those of a group of men having the same aims, were as good, or as true, as those of any other group or individual.

There have been, however, certain developments in science which may promise a surer, if slower, way out of some of these difficulties than the one the philosophers have followed.

In the *Scientific Monthly* for September 1944, two articles appeared, in which Claude Bernard's principle, which later came to be termed 'homeostasis,' was used as means of explanation. S. J. Holmes, writing on the 'Problem of organic form,' quotes the great physiologist as expressing the principle in these words: "All the vital mechanisms, however varied they may be, have only one object, that of preserving constant the conditions of life in the internal environment." He also quotes H. S. Haldane, who declared that "No more pregnant sentence was ever formed by a physiologist" (8, p. 228).

The other article, by Samuel Brody, is entitled, 'Science and social wisdom.' Mr. Brody defined the purpose of his article as follows: "The following discussion attempts to formulate this problem of social disturbance and self-correction from the viewpoint of what may be called the theorem of Claude Bernard, in biology, analogous to theorem of Le Chatelier in physical chemistry. Both theorems are concerned with broad factors tending to restore disturbed equilibria, one in biologic, the other in physical science" (4, p. 203).

Mr. Brody, with apt illustration and argument, maintains that not only is this principle important in the maintenance and recovery of bodily health in the individual, in the long-range survival of the species, and in the coöpera-

tion necessary to the continued survival of a society of lower animals, but also that it is of prime importance to moral, political, and religious welfare of a society of human beings. Upon reading Mr. Brody's article, it seemed as if a further application of this principle might, in the long run, serve to open certain questions as to the 'original certainty, and extent of human knowledge' (13, p. 244) to empirical investigation and experimental research; and that Claude Bernard's principle was immediately applicable to trial and error learning, and, in fact, filled a gap in the current explanations of such learning. It was, moreover, a specific application of a property common to field theories, *viz.*, that, upon being disturbed, each field tends to regain its proper pattern.

Let us assume, then, that each living organism exists within the natural order; and, insofar as it possesses corporeal being, is acted upon, is interacting with, and is acting by means of, all those things, forces, and agencies—physical, chemical, biological, psychological, social—which constitute its environment and go to make up its existence. By this environment it is surrounded and engulfed, and it, itself, forms a part of the environment of other creatures. In nature there is no such thing as an external observer; but intelligent beings exist and develop within the natural order. Moreover, insofar as our present knowledge goes, psychic processes are to be found and studied empirically only in relation to the activities of individual living organisms. Furthermore, they seem particularly to be connected with the preservation of the integrity of such an organism and its behavior as a whole. If one studies an organism engaged in learning, one will find that, in the course of time, something of a dynamic adjustment is attained between its behavior pattern and the patterns of events which constitute the environment in which it lives. Thus it seems as natural a process for the animal's mind to acquire knowledge as for its stomach to digest food. In the case of food, matter and energy are added, are built up into the living substance of the organism, and are used in carrying out its activities. In the case of knowledge, neither matter nor energy is added, but the animal's behavior pattern is modified, or potentially modified; and, in general, rendered more effective in preserving its integrity and existence. Thus, understanding has been defined as a becoming, a modification of the organism in conformity with the object, or, more generally, with the patterns of the environment and its relations to it. In coming to know, the animal has acquired a more effective dynamic form.

An animal lives and learns by maintaining and developing a certain relationship of correspondence, conformity, or harmony between its own activities, which are a real factor in every situation, and the forms, positions, actions, and potentialities of the forces, things, and other living beings outside. A working agreement must somehow be reached between the character of the external entities with which it is concerned and the vital activities of the organism itself. Learning, then, is one of the important methods by which such harmony and adjustment is brought about. The animal's activities are in part concerned with and controlled by the things outside, but are always conditioned by the drive, form, and preservation of the life within. Homeostasis, the tendency of all vital mechanisms to preserve constant the conditions of life in the internal environment, would thus appear to be a basis factor in the ability to learn.

Intelligence, or the ability to learn, begins very far down in the scale of life. H. S. Jennings, in his *Behavior of*

the Lower Organisms (12), which was published first in 1906, and has remained a minor classic of its kind, shows that even the protozoa are able to so modify their behavior as to keep out of, or at least to get out of, unfavorable regions of stimulation, and thus to remain in favorable ones. They do so by a kind of trial and error process, not different, in principle at least, from the trial and error processes followed by all the higher animals, including man.

For example, *Paramecium,* the slipper-shaped animalcule, swims by means of cilia, which cause it to progress through the water in a spiral course, with a more or less straight axis. It may thus proceed directly from place to place. The cilia reverse their beat when the animal is stimulated. *Paramecium* then shoots back, swings its anterior end to the left, and swims forward on a different course. If, by chance, this new course takes the animal out of the region where the stimulating force is acting, it continues on its way; if not, the reaction is repeated, and a new direction tried, until one is found by which the animal escapes from the region of the stimulating force. If the stimulus is too severe, and there is no way out, the animal may continue dashing frantically about until it dies. *Paramecium* reacts negatively to salts and positively to weak solutions of acids. It does not react on entering a region of weak acid solution, but does react upon leaving it, and thus remains within it. Other paramecia swim in from the outside, and a cluster forms. Or a gradually extending ring may be formed around a drop of weak acid diffusing through the water. That is, if the concentration of acid is too strong, so as to disturb its life processes, the animal reacts negatively, and thus keeps out of too strong a concentration. On the other hand, if it swims in such a direction as to leave the zone of the diffusing acid, the ordinary culture water then acts as a stimulus. The animal responds with the avoiding reaction, as has just been indicated, and thus remains within the zone of such concentration of the acid as apparently does not disturb its life processes, or homeostatic condition, and in which such processes seem to proceed in their normal manner. It will be observed, however, that some, at least temporary, change has taken place in the animal's internal economy so as to modify its behavior pattern to the extent that it now reacts negatively to the culture water which, previous to its entering the zone of the diffusing acid, did not cause it to react negatively (12, pp. 41–109). This would seem to be a dawning of the ability to learn.

In the case of *Stentor,* the trumpet-shaped infusorian, an additional principle is well illustrated, which is important for an understanding of the ability to learn. While this animal is quite able to swim about, it is more often found attached to solid objects by its pointed end, or foot, with the lower half of its body surrounded by a tube or sheath. Thus is an irregular mass of mucus secreted by the animal, and usually containing debris, bits of vegetable matter, grains of sand, etc. It feeds by causing currents of water to pass, by the beating of its cilia, over its mouth, which is located at the upper, or larger end of its trumpet-shaped body, and near the center of a kind of vortex formed by these water currents.

Jennings, after giving an account of several experiments in which temporary adaptive modifications of behavior were observed, tells of one in which a cloud of carmine grains continued to be added to the water which the beating cilia of *Stentor* were bringing to its mouth. At first *Stentor* did not react. Then it reacted by changing its position. This, however, did not prevent the carmine

from still being drawn into its mouth. Then came a reversal of the beat of the cilia, which drove the carmine grains away momentarily, but with the renewal of its usual feeding activities, more carmine was drawn in. Then the animal broke off its normal feeding activities entirely, by contracting into its tube. Finally, the cloud of carmine grains continuing to pour down upon it and into its mouth whenever it attempted to feed, it abandoned its tube, swam away, and made another one in a place where the carmine did not pour down upon it (12, pp. 170–179). In discussing these reactions, Jennings says that different responses may be made to the same stimulus, due to differences in the physiological state of the organism, and there are variations in the details of the reaction series, "but it remains true that under conditions which gradually interfere with the normal activities of the organism, the behavior consists in 'trying' successively different reactions, till one is found that affords relief" (12, p. 177).

The principle of the readier resolution of physiological states is also illustrated here and is of great importance in understanding the formation of habits and the ability to learn. It may be stated as follows: Under certain circumstances an organism reacts in a certain way. These conditions continuing, the organism changes its first reaction for a second, a third, or a fourth, as A to B to C and finally to D which affords relief. But, it is frequently found that, after many repetitions the first condition A is followed immediately by D. And further, it is found that such resolution of one physiological state to another, as of A into D, occurs the more rapidly and more certainly after it has taken place a number of times (12, pp. 289–292).

In a typical instance of regulatory behavior, the tranquil functioning of the organism is disturbed by the action of a stimulating force. This produces a state of tension, or disturbance within the organism, and is followed by 'trial and error' activities. One of these brings relief to the tense conditions and a return to normal functioning. If the situation occur frequently enough, and the same solution repeatedly is found, a new and useful habit may be formed, which enables the organism to deal with like situations more promptly and effectively. These results are substantiated by many observations, experiments, and a host of facts, and have been confirmed by many observers.

The question naturally arises, "Are these lowly organisms conscious?" In absence of proof, but reasoning by analogy and on the basis of the observed facts, it would seem likely that primitive forms of consciousness are present among the lower organisms. Apparently feelings of enjoyment or pleasure accompany the maintenance of a unified dynamic state of the organism as an integrated whole. Tension, or motor conflict and displeasure (9, p. 226), accompany interference with this integrated dynamic state; and relaxation of tension with pleasure again, if, by trial and error activities, or by some other means, the action of the disturbing stimulus be removed, and the vital mechanisms again achieve their object of preserving constant the conditions of life in the internal environment. It is evident that the forms of consciousness become more and more varied, complex, differentiated, patterned and elaborately integrated, as one goes up the scale of animal life from the protozoa to man.

The principles of such intelligent action as may be witnessed among the lower forms of life are remarkably similar to those expressed by the pragmatic philosophy, and it is puzzling to know

why the pragmatists in general have not shown more appreciation of Mr. Jennings' admirable book. In all fairness, someone should put in a claim for *Paramecium* as being the first-known pragmatist, anticipating the claims of Messrs. Peirce, James, and Dewey by some eons of geologic time.

There is one common difficulty, however, in that neither Professor Jennings nor the pragmatists seem to know exactly what they mean by 'healthy activities,' 'normal functioning,' or 'an optimal condition of stimulation,' to use the language of the one; nor by 'an end which emerges as we go along and a future type of society in which man will be better adjusted to his surroundings than at present,' to use the phraseology characteristic of the others. Each of us may, of course, recall how Professor James wrestles with this problem in his *Pragmatism* (10). An idea is true if it works. But what do we mean by 'works'? Cash value, harmony with other propositions, higher values, all these are suggested severally and collectively, but all this does not seem to convey any clearer idea than does the word 'works.' These are, of course, value judgments, and in part judgments of relative values, but there is no standard nor scale by which they may be judged, measured, or compared. Nor is it evident, without going beyond the principles of the pragmatic philosophy, how such a scale could be set up and maintained. For, as the scholastics used to say, "more or less are predicted of different things in so far as they approach that which is most." Thus, for example, one ball is rounder than another the more closely it approximates a sphere. But there are no 'mosts' in pragmatism; nor is there any basis for calibrating whatever is meant by 'works.' For how can we say that one thing or idea works more, or less well, than another, if we do not know what we mean by 'works'? Does death 'work' better than life, or life 'work' better than death? Is the recovery of health a process which 'works,' or is progress of disease something which 'works'? Was it the rise, or was it the fall, of the Roman Empire which 'worked'? Or did each 'work' according to its lights? Does a static thing 'work' which maintains itself intact? Or a dynamic thing which maintains in being its pattern of movement? Or does a highly complex being, a plant or an animal, 'work' if it maintains its organic integrity from birth to death, through a life cycle of growth, maturity, and decline? Are we to conclude, then, that what 'works' for any being is that which is in accordance with its nature? We might have a standard, then, assuming we could determine what its nature was, and thus be able to tell by how much each varied from the standard. For one ball is rounder than another insofar as it approximates the more closely to a sphere. But, in setting up such a standard, we would seem to be among the followers of Aristotle or St. Thomas now, and to have left the disciples of James and Dewey.

The physicists and engineers, being Platonists according to their actions, although not always according to their words, are more fortunate, since they have their 'mathematical things' to set up as clear-cut standards and norms. The physicist's measurements, within due margin of probable error, must correspond to the ratios inherent in the formulas he employs. In Plato's language, they 'participate' in these forms. If the two do not correspond, the physicist assumes either that his measurements are inaccurate, or that he has chosen the wrong formula, or has made some other merely human error. He does not assume that the forms themselves are wrong. Likewise, the engineer, by comparisons with the 'mathe-

matical things,' can tell, within extremely narrow limits, by how much an automobile cylinder lacks of perfection of form and size, or, within still closer limits, how much error still remains to be eliminated in the grinding of a lens. Mathematics, says Bertrand Russell, takes us "into a region of absolute necessity, to which not only the actual world, but every possible world must conform" (14, p. 69).

Biologists and psychologists, unfortunately, have, as yet, in connection with the problem here being discussed, no such subsistent entities to set up as standards or norms, and must therefore try to find some existent processes which may serve to distinguish the healthy from the unhealthy; that which 'works' from that which does not 'work'; the normal from the abnormal or pathological, in body and in mind. In still more general terms, this would mean discovering some standard process, or processes, to distinguish that which is proper to a living being from that which is not in accordance with its nature. This would involve the pattern of the organism as a whole, as well as the patterns of its parts, for the parts might each be properly organized, but, if not combined properly into the whole, the organism would lack the integrity proper to it. Regarding the individual living beings of any particular species, it would seem that those vital processes which exemplify the principle of homeostasis must continue active if the animal is to continue to exist. And the integrity of the parts, as well as that of the organism as a whole, must in large measure be preserved, if the animal is to remain alive in its natural environment and be able to meet its demands upon it. Thus, the integrity of the organism as a whole, and especially that of the conditions of life in the internal environment, would constitute the normal condition of the organism, while departures from it, in divers ways, and to different degrees, would constitute abnormal ones.

Some of the more recent biological work on the subject of integrity is of interest in this connection. Among other books and articles is one entitled *Levels of Integration in Biological and Social Systems*. It is a symposium, in which ten authors, including Professor Jennings, express their views as to what constitutes the unity, wholeness, or integration of corporeal substances in general; and then, more specifically, what constitutes the integrity of a living being; and, finally, that of a society of living beings and the means observed to maintain the integrity of such organisms.

R. W. Gerard, in an article in the book just mentioned, entitled 'Higher levels of integration,' distinguishes, within the genus of organized beings, three species which he delights to honor by the euphoneous terms of "org," "animorg," and "epiorganism." An org is a unit system, composed of lesser units as its parts, in which reciprocal influences exist between the parts and the whole. Orgs differ in two general ways: degree of integration and level of organization. A more integrated org represents a greater degree of control of the units by the whole as compared to the control of the whole by the parts. Thus a group of molecules in a gas are hardly integrated into an org at all. In a liquid they form a slightly more integrated collection. A second characteristic of advancing integration is a general differentiation of constituent units. Another distinct stage is reached when things at one level of integration form the constituent units of things at a higher level of integration.

Living organisms, or animorgs, form a species within the genus of organized being, which is easily distinguished by certain of its characteristics, but not so

easily defined. "The panorama of evolution," Mr. Gerard continues, "shows a strong tendency for animorgs, indeed for orgs in general, to evolve toward increasing integration at any one level, and then to superimpose additional levels of complexity" (6, p. 76). And he goes on to say that at the subcellular level inanimate beings increase in integration until autocatalytic and virus molecules appear. The cellular level is then added, with molecules as units, and at this new level integration again increases from the simplest micrococci to the most elaborate flagellates. Another level, that of multicellularity, is then added and the metazoa increase in complexity from the sponge to man. Nor does the process stop here, but epiorganisms, or societies composed of living individuals, arise. The social insects evolve from crude and temporary groups into the highly characterized epiorganisms of the single beehive, ant hill, or termites' nest. The physical characteristics of the nest are unique for each species, yet the nest is the collective accomplishment of the group, and quite beyond the capacities or level of the individual. In the same way the mammalian family, the herd, and ultimately, the larger social groups, constitute various societies. In human affairs the same law holds, from the single nomad tribe to the British Empire (6, pp. 74–77).

Regulation of behavior, then, with the learning process as one of its component activities, particularly as regards living beings and societies, would seem to mean those activities which accomplish, or which tend to accomplish, the preservation of the integrity of individual beings and, analogously, that of societies composed of individual living beings. It is the animal's task to preserve its integrity under changing conditions, internal or external, and in so doing, to avoid disease, injury, or death. Moreover, if an organism tends to make itself survive, it tends to survive and to secure all the biological advantages of the survival of the more fit. However, it is the specific character of its integrity, the particular pattern of its being, as well as the character of its environment, which determines what actions it may, or may not, advantageously perform. And its integrity is something pertaining to the organism itself. It is not a part of the environment. Although limited by, and dependent upon, environing circumstances, resources, and conditions, its own nature and the integrity of its own being are the prime factors in determining what activities are intelligent for it to perform, and what the stupidities are which it may commit. "Everything which has a nature acts in a way determined by its form, according to the law of its being, and that action is good in so far as it tends to the perfection or full activity of the nature" (5, p. 228).

A living being is not only 'something happening,' it is also of the nature of an event, with a beginning, a middle, and an end; and tends to accomplish and maintain its dynamic integrity under changing conditions within and without. Definite activities are taking place within it, in accordance with its structure and mode of life, which tend to preserve the integrity of the whole, but especially that of the conditions of life in the internal environment. If then, as Claude Bernard says, "all the vital mechanisms, however varied they may be, have only one object, that of preserving constant the conditions of life in the internal environment" (8, p. 228), this constant dynamic condition of the most important and vital activities of a living being will constantly tend to bring back into line with it such changes of activity as may run counter to it or deflect it; whether such changes be brought about directly, by some ex-

ternal or internal stimulation, or indirectly by some change in the conditions, or in the rhythm of the bodily processes themselves.

As Mr. Brody says, "Just as the field in physics may be conceived to be a physical (an electro-magnetic) integrative process, so the field in biology may be conceived to be an integrative biologic process binding many components into a whole and tending to be restored on disturbance, which is one definition of homeostasis; the tendency to be restored to normal upon disturbance. The organismic view leads to the inference that in the struggle for survival in the course of evolution, the component parts of the body had to develop so as to be in symphonic harmony of an optimal pattern. An organism is a close-knit community, the component members of which—nervous, endocrine, circulatory, excretory, digestive, and so on—coöperate in maintaining a dynamic steady state in the face of fluctuating external conditions. This is essentially Claude Bernard's generalization, in modern language" (4, p. 205).

Thus, it is evident that homeostasis, that class or species of integrative process to be found in living beings, forms a part of a larger class, or genus, of integrative processes to be found in both living and non-living beings. All integrative processes are characterized by the combination of particular activities into a single whole, organized upon an intelligible pattern, and by the tendency to revert to this same structural and dynamic pattern, should its condition or activities be disturbed.

In the living organism the return to pattern is a return to something intrinsic to the organism itself, and not merely an adaptation to the environment, in the sense of merely fitting in with something outside itself. Harmony between organism and environment is a two-way relationship, and becomes more definitely so as living beings become able to modify the effects of environmental forces upon themselves, to learn to take advantage of them, and finally, in the case of man, to exercise a certain degree of control over them. Moreover, the return to normal of conditions within a living organism is not merely a return to the *status quo ante,* but rather to that of the *motus quo ante;* that is, to something that is not merely going on, but is already on its way, in accordance with the growth, development, and life cycle of the organism itself. It is not merely the reëstablishment of a process which is changing in accordance with a rule. For there are both the current activities of the organism, and also, combining with these, the changes determined by its ontological development and growth. Although the product of evolutionary processes, and conditioned by environmental circumstances, the individual organism seems largely determined from within in its tendency to realize its specific space-time form. Thus a mutant variety of an established species will follow a cycle of life and a general pattern of behavior whose differences to those of its parents are very largely determined by the difference between its genes and those of the parent strain. And it will survive or perish in the struggle for existence, accordingly as it is determined from within to be more able, or equally able, or less able than the parent variety to meet, or to learn to adapt itself to, similar environmental conditions. These traits of living organisms approximate to Aristotle's designation of beings produced by nature, as distinguished from those produced by artificial means, since none of the latter "has within itself the source of its own production" (2, p. 192b). For these traits, taken together, largely determine the structure, activities, growth, and development of each organism. And it

is upon these as a basis that its ability to learn is founded.

If, then, the dynamic integrity of the organism as whole furnishes, at least, a biological standard of what 'works,' it may be possible to explain trial and error learning somewhat as follows:

Each living organism, in relation to internal as well as to external changes and conditions, tends to maintain itself as an integrated whole, as do also social organisms and a wide range of inanimate things. Each is, then, not merely 'something happening,' but is a complex, integrated, and unified system of activities. Thus, definite internal changes are taking place within each living being in accordance with its character and mode of life; and all its vital mechanisms, however varied, combine to maintain a uniform dynamic state, or 'field,' within each individual, in the face of fluctuating conditions of internal or external stimulation.

Internal or external conditions of stimulation which interfere with this homeostatic condition, or otherwise cause tensions and disturbances within the organism, are followed by varied movements.

When, through one of these varied movements, conditions which do not interfere with the processes proper to the organism are attained, the change from one movement to another tends to cease, and that movement, or activity, continues through which the tension or disturbance is relieved, and the field is permitted to return to its proper pattern.

Fixation of such adaptive movements, through the law of the readier resolution of physiological states, ordinarily occurs. When this has taken place, we say that the organism has learned by trial and error. A new and useful proclivity, and, in time, a habit, may thus be formed, enabling the organism to deal with like situations more promptly and effectively in the future.

It would seem unnecessary to multiply illustrative examples, or to cite an array of instances of Claude Bernard's well established principle. But the following would seem to indicate its presence, as a factor, in certain cases which seem to lie on the border line between what is ordinarily termed intelligence and what might be termed merely bodily adaptation.

The recovery from mental and bodily diseases and the healing of mental and bodily traumatic conditions may be dependent on how far the tendency of the organism to return to normal can be successfully completed from within, or with possible aid from without. The body seems able to adapt itself to new conditions, to a certain extent, by changing its physiological reactions to meet them. Thus, changes in diet, climate, and living conditions, which at first cause considerable tension and bodily disorders, may be met by changes in the economy of the body, which in time establish a considerable degree of harmony between the novel action of the external agencies and the conditions of life in the internal environment. Thus, Pavlov found that the pancreatic juice of dogs, which is naturally well adapted to the digestion of meat, could be changed so as to digest large quantities of starch if the dog were maintained on a very starchy diet for some weeks, but could not be easily changed back again. We may come to enjoy what at first we hated. Alcoholism and drug habits seem to indicate a condition in which the human organism has so adjusted itself to the effects of these substances upon it as to be far less disturbed by them than it was in the beginning; but curiously enough, it is disturbed by them in such a manner that now to be deprived of alcohol or of the accustomed drug may

cause a very considerable degree of tension, desire, and craving, and may, in extreme cases, bring about dangerous disturbances in the condition of the organism. These seem examples also of the body's ability to learn.

If, then, intelligent beings exist and develop within the natural order, it would seem as if intelligence must be potential in an earlier or more primitive series, or pattern of events, if it is to become actual in a later one. Is it an emergent, or the actuality of something definitely potential in life itself? One may observe the passage of intelligence from potency to act in the ontological development from fertilized egg to maturity in the case of a human being or of an animal. One may study it in particular cases, and even, as Carmichael has been doing, derive some generalized forms of knowledge from it in regard to innate and acquired patterns of behavior. Jennings' discussion of chemical and form regulation as being due to the same process whose operation at later stages is called intelligence, at least gives food for reflection and indicates an important field for study and research (12, pp. 343–349). There would seem to be a series of many borderline cases representing degrees of difference between what is definitely conscious and what is apparently quite unconscious regulatory behavior. As for example, the painless healing of a wound, once the pain of the initial injury has passed. Certain lacks, from food to holiness, may cause feelings of tension and frustration within the human organism, but without a clear knowledge of their cause. In some forms of insanity, or of cases bordering upon insanity, it would seem as if homeostatic conditions within the body were, as far as possible, maintained at the cost of rendering the person so afflicted incapable of reacting efficiently to the demands of the external environment, or even quite incapable of answering to those demands. He may shut out, or withdraw from, the external world, or even from parts of the internal world, by inhibiting the recall of certain experiences whose appearance in memory continues loathsome to him; or he may seek protection by simulated blindness, deafness, lameness, etc., against particular things with which, in his present condition, he feels he cannot cope. By shuffling off part, or the whole, of its relations to a world outside, the organism endeavors to preserve something of its integrity within.

Elasticity, the inertia of bodies, and the tendency of a field to return to its proper pattern upon disturbance, are to be found wide-spread in nature. In this manner, many 'orgs' tend to preserve their integrity, and, by resisting the action of outside agencies, maintain themselves in being. Apparently the inanimate things of nature do not learn, although intelligible, they are not intelligent. But the atoms of the simpler elements show enormous resistance to the action of natural forces upon them, and, unless subjected to conditions comparable to those within the stars, retain their integrity throughout enormous reaches of time. However, at present, certain complex elements can be artificially broken down. They loose energy in the process, as the atomic bomb may illustrate, and the remainder of their constituent entities fall into simpler patterns and thus form simpler elements. Compound chemical substances are, in general, more easily broken down into their constituent atoms; while colloids and viruses lose integrity still more readily. Analogous processes occur all along the line upward. Individual organisms die and cease to exist upon the completion of their life cycle; or, when, because of injury or disease, they can no longer maintain their integrity, *i.e.*, their essen-

tial form of being and pattern of activities.

Regulatory behavior is a type of activity which renders it possible for a complex being to avoid merely continuing in a state of motion or of rest in the face of injurious or destructive conditions; but, by making certain modifications in its behavior, to continue to exist as an integrated whole. Trial and error learning is an early form of such behavior. It appears among the lower organisms and seems due to a combination of factors. Each of these factors may be found to exist in inorganic nature, but a functioning combination of these factors, so far as is known at present, is found only in living beings. Some of these factors may be found artificially combined in automatic pilots, steering gears, and in a number of similar devices, but here also no artificial product "has in itself the source of its own production" (2, p. 192b). However, it would seem to be quite possible to devise a gadget which would 'learn,' in much the same sense as that in which a calculating machine 'calculates.'

It is obvious that the trial and error type of learning, the form of knowledge with which we have chiefly been concerned, is very directly linked with those activities which maintain the animal in being and which tend to promote its welfare. The lower organisms seem to have room for but a minimum knowledge of the patterns, structures, and tropes of a world external to themselves, and likewise to possess no great variety of feeling. However, as one goes further up the scale of being, one finds, in addition to trial and error, other types of learning which have to do, not only with the animal's immediate welfare, but also with its particular type of reactions to the forces which play upon it, to its relations to external objects, and, eventually, to the patterns and movements of these objects themselves. Knowledge of the patterns and forms of things originates and continues to grow in closest connection with the animal's other activities. Not only is such acquisition checked and guided by the ever-present homeostatic action of its internal economy, but it grows through, and by means of, the actions and reactions of the animal as it comes in contact with, and otherwise in relation to, the spatial and temporal patterns of the things, forces, and activities outside. This it does in seeking food and shelter, in the propagation of its species, in pursuit and escape, attack and defence, and in many similar ways; also in the formation of habits and the acquisition of skills, and in acquiring various new types of action characteristic of the different stages in its development and growth. Thus it is in action that contours are noted and the spatial and temporal patterns of things utilized and made familiar.

Intelligence thus appears among the lower organisms in the guise of trial and error behavior, and perhaps a simple type of consciousness accompanies such activities. Habits are formed on the basis of trial and error activities, and the behavior pattern of the organism thus modified. There are, however, certain limits to such modification since the vital mechanisms combine to preserve constant the conditions of life in the internal environment and to resist departures from them. In addition to this, disease, injury, and death tend to prevent a living being from straying too far from the straight and narrow path which evolution, heredity, and growth have marked out for it to walk in, if it is to preserve its integrity and maintain its welfare. Its phylogenetic evolution has determined all but completely its anatomical structure and the vast bulk of its physiological reactions. Within the range of possibilities which remain, the stage of its ontological development

determines still further which set of the activities proper to it it shall tend to exercise at any particular stage of its career. Thus, the range of possible habit formation is considerably narrowed down by these hereditary factors; and, if the environment is stable, animals of the same species and sex behave with considerable uniformity in vital matters, but may manifest many individual differences in less important ones, particularly in those which do not seriously disturb homeostatic conditions whether such individual differences are acquired by the formation of habits or by other forms of learning. The development of the nervous system, which appears with the metazoa, renders more and more effective, and elaborate, the modes of regulatory behavior, from the coelenterates to man.

Societies, or organized groups of animals or men, behave somewhat analogously to individual living beings. Becoming disorganized, they, too, break down into their component parts, and may continue breaking down until such units are reached as are able to maintain themselves as organized wholes under their environing conditions.

In all higher forms of mental life the pragmatic, trial and error, type of learning is a constantly present factor, and patterns of behavior acquired by such means tend, quite directly, to the preservation of the integrity of the organism. But, very obviously, it is not the only type of knowledge acquired by the higher animals and by man. Both actually and potentially, knowledge of the forms, positions, and movements of things, and of other living beings, and of how to move about among them, is of the greatest use to an animal, and not infrequently a matter of life and death. Curiosity, or mere contemplation, may furnish, in many instances, more knowledge than can immediately be put to use. The surplus is not all lost, nor immediately forgotten, however, but some of it is retained, remembered, or otherwise made subject to recall, and may be very useful later on, forming, with its tendencies to action, a part of the apperception mass, which links up with the incoming impressions and determines, in large measure, the animal's experience and behavior in the presence of such things as those with which it has had experience before.

Sensation, perception and insight, imagery and the association of ideas, and finally reason, may roughly indicate the main stages in the evolution of intelligence from protozoa to man. The lower forms of intelligence are retained in the higher organisms, the higher forms being superimposed upon them. Thus human beings possess all the general ways of learning possessed by the animal mind, and, in addition, those which are distinctively human and rational, and, most specifically, abstract conceptual knowledge.

In man, abstract conceptual knowledge, of which scientific knowledge forms a part, arises when attention is attracted by some quality, aspect, or relationship which is common to several things, entities, events, or situations, in other respects differing from one another. His attention being focused on what is common to all, he is prone to disregard what is different in each, and to become oblivious to all else but that upon which his mind is concentrated. Furthermore, an investigator, being set to follow one common characteristic through its various exemplifications, tends to disregard other characteristics, which may also be present. And, even if these are noticed later by the same, or by other investigators isolated in thought from the other components and expressed in abstract conceptual form, the logical relationships between the various concepts may be anything but obvious.

The result is, as Abelard stated in the earlier half of the twelfth century, that man's conceptual knowledge is limited to being 'faithful but incomplete.' Abelard says, "I know nothing but what is in the thing, but I do not know all that it possesses." "If I were to deny anything the thing possesses, my notion would be false, but this is not the case." (*Intellectus per abstractionem divisium attendit, non divisa, alioquin cassus esset*") (1, p. 164). Thus the activities of trial and error learning described in this article may truly tell the manner in which living beings behave when learning by trial and error, but is not intended to imply that there is nothing to the living beings and their activities but the behavior pattern described therein. The reality of a single being may be the object of multiple concepts, each one 'faithful but incomplete.' Do we need to remind ourselves that Abelard uncovered the 'nothing but' fallacy some eight centuries ago?

If the complexity of a situation becomes so great as to be beyond the power of organization of our thoughts, we are apt to go downward, in the search for a solution, through the various subrational stages of feeling and behavior, until we may end with very little else than trial and error, activated or controlled by some emotional drive. When all else fails, there is always the primitive (7, pp. 377–378).

Intelligence begins in the service of, and in closest relation to, the needs and essential activities of organisms. It can never cease for long, and upon this earth can never entirely cease, from being engaged in such activities. But it may be engaged in other activities also, which, in the end, may be greatly beneficial, although they bring in no immediate return; and, like Aristotle's state, which likewise originates in the bare needs of life, "it may continue to exist for the sake of a good life" (3, p. 1280a).

In man, certain superfluous forms of knowledge tend, so to speak, to take on existence of their own, and knowledge to be pursued for its own sake. At the level of reason and conceptual thought and joined to a certain attitude of contemplation, the pursuit of knowledge for its own sake has given rise to philosophy and the sciences, and systematic applications of science to the technologies. It is not necessary, however, for knowledge to be entirely disinterested in order to be true. If it were, few animals or men would live out their appointed span. But it is not difficult to imagine how much a knowledge of the forms and relationships of things, and of the activities of other living beings, would contribute to the preservation of an animal's welfare and its ability to survive. The higher animals come to possess a considerable amount of such knowledge in terms of perception and memory. Man, in addition to these, has a very highly developed speech; abstract, universal, and conceptual forms of thought, with their sustaining technical symbolism and terms; the ability to determine causes and effects, and to draw rational inferences. All this tends to make the patterns in the world of nature manifest themselves to him with greater clarity, extent, and articulateness, and to make the relations discernible between such patterns better understood. These are the ends of knowledge, but knowledge, especially of that which is highest, may be the end of life.

The germs of what later become articulated forms of knowledge are, however, to be found among the lower organisms. *Paramecium* reacts positively to acids and negatively to alkalis, and thus, in its behavior, distinguishes between them (12, p. 304). Many of these lowly organisms are observed to

react to a low intensity of a stimulating force which, in itself, does them neither harm nor good, but which is usually followed by so strong an intensity of that same force as to be either injurious or beneficial. The weaker stimulus acts as a sign of the coming of a stronger one of the same kind. A clearer case of the existence of the symbolic type of response among the lower organisms is that of the sea urchin, which reacts to a shadow falling upon it by taking an attitude of defense (12, pp. 296–299). In this case, the symbol, a shadow, and the thing signified, an enemy-about-to-attack, are things different in kind. Such symbolic activities are very important in the further progress and development of intelligent and, finally, of rational behavior.

Perceptions and memories thus acquire meaning and become charged with motor impulses. But the behavior, and the results of the behavior, to which they lead, must conform to the patterns and movements of external things, on the one hand, and tend, in general, to maintain the integrity of the organism, on the other. This process has, of course, been going on for many million years, and now, as William James said, "Mind and the world in short have been evolved together, and in consequence are something of a mutual fit" (11, p. 4).

One of the processes by which this 'something of a mutual fit' is brought about, in living organisms, is trial and error learning. An effective working combination of the following factors would seem to be essential to such learning. (1) A unified dynamic 'field,' or homeostatic condition, within each organism, which constantly tends to resume its proper pattern of activity, in the face of fluctuating conditions of internal and external stimulation. (2) Stimuli which definitely cause disturbance within this field are followed by a succession of activities, which bring about various results. (3) This succession of changing reactions ceases, if one of them causes the stimulation to cease, and thus permits the field to resume its proper pattern. (4) Upon sufficient repetition of the situation, and of the activities called out by it, that reaction which causes the end of the disturbance within the field is found to follow, with a very considerable degree of regularity, immediately upon the action of the stimulus. Thus a habit is formed, which enables the animal to deal with like situations more promptly and effectively. We then say that the animal has learned by trial and error. It seems evident that the presence of a homeostatic field, or condition, in which "all the vital mechanisms, however varied they may be, have only one object, that of preserving constant the conditions of life in the internal environment," is an essential factor in trial and error learning by living organisms.

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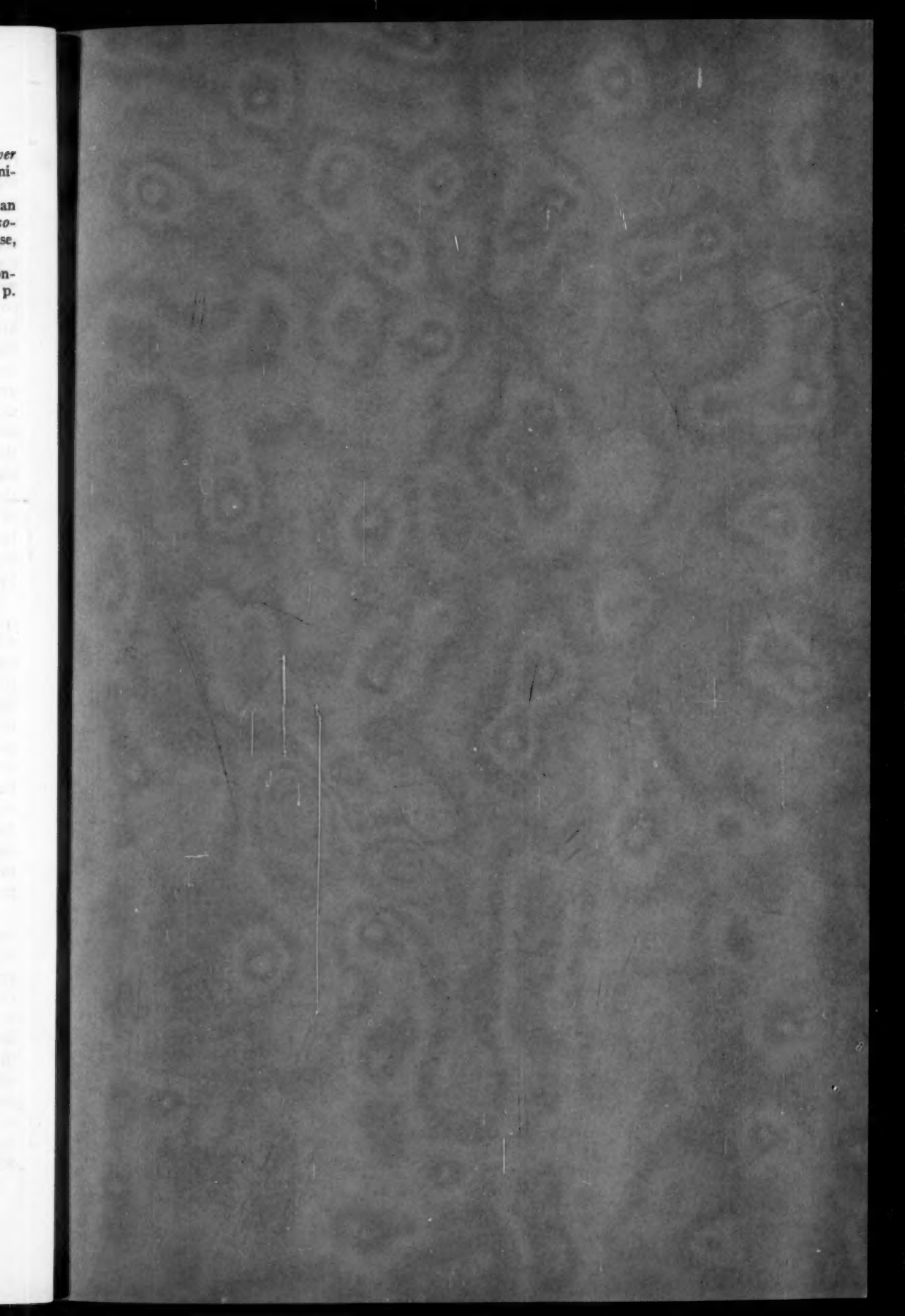

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